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ARITHMETIC AND REALITY

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I propose to discuss whether "queer" arithmetics (to be thought of on the analogy with non-Euclidean geometries) can be effectively applied to describe the world. Since this question has been investigated at length and quite recently by Professor Douglas Gasking in his very famous and influential paper "Mathematics and the World"[1], the first part of my discussion is devoted to an examination of Gasking's views and arguments.

I

A Conventionalistic View of Arithmetic

1. *Gasking's view.* Professor Gasking has argued most persuasively for the view that mathematical propositions are like conventions or rules as to how we should describe what happens in the world. The proposition "$7 + 5 = 12$", e.g., "does *lay it* down, so to speak, that *if* on counting $7 + 5$ you do get 11, you are to describe what has happened in some such manner as this: *Either* 'I have made a mistake in my counting' *or* 'Someone has played a practical joke and abstracted one of the objects when I was not looking' *or* 'Two of the objects have coalesced' *or* 'One of the objects has disappeared', etc." (p. 209).

Gasking's argument is non-technical and seems very clear and, therefore, very convincing. As far as I can see his view may be summarized in the following theses:

(A) Mathematical propositions are incorrigible (p. 207, etc.);

(B) They are not descriptive of the world (pp. 208 ff);

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[1] This *Journal,* 1940; reprinted in Flew, *Logic and Language,* 2nd. series (Oxford: Basil Blackwell, 1953), pp. 204-221. Three-figured numbers in parentheses indicate pages from the latter.

(C) Their truth depends on the usage of symbols only (p. 217) but (C1) they are not simply rules for the manipulation of symbols (p. 210);

(D) their characteristic function is to formulate a way of calculating (p. 219);

(E) such a way of calculating together with a way of counting and a way of measuring constitute criteria for our descriptions of the world, i.e., arithmetical propositions are some of the rules governing our describing of reality (p. 219);

(F) there is no necessity in the boundaries separating calculations, countings, measurements, and facts; we can very well draw different contours for, say, arithmetic provided we modify accordingly our countings or measurements or our descriptions of fact (pp. 219 ff).

Of course, it is unfair to represent Gasking as merely making these seven assertions. The point of his paper is to explain them as well as to argue for them. Thus, he argues to (B) from (A), and spends one third of the essay in proving (F), from which he infers (C). He speaks of queer arithmetics for what I have called different contours of arithmetic (cf. Part II below). He discusses the example of an arithmetic in which "$3 \times 4 = 24$", and argues that we could employ it quite successfully in our ordinary problems like paving a floor.

It should be said that Gasking makes claims about mathematical propositions in general, even though his actual discussion is limited to "$3 \times 4 = 12$", "$3 \times 4 = 24$", "$7 + 5 = 12$".

2. *Incorrigibility.* Gasking defines: "A corrigible proposition is one that you would withdraw and admit to be false if certain things happened in the world" (208). And most people would agree with him that mathematical propositions are incorrigible in that sense. Clearly, "$7 + 5 = 12$" tells us nothing about events, so that "no future happening whatsoever would ever prove the proposition false, or cause anyone to withdraw it" (207).

Then Gasking goes on to say that only corrigible propositions can be informative or descriptive of the world. But he seems to assume a definition of 'informative' or 'descriptive of the world' in terms of corrigibility: "A corrigible proposition . . . therefore gives you the information that *those* things (i.e., those things which would make you withdraw your proposition if they happened) *will* not happen" (208). Certainly, if that is what 'descriptive of the world' means, mathematical propositions are absolutely uninformative or nondescriptive. However, the term 'descriptive of the world' is often used in another sense, in which to say that incorrigible propositions or statements are not descriptive of the world is, among other things, to beg the question against Kant, whether there is a synthetic *a priori* or not. In fact, Gasking does continue his argument, and his defence of thesis (F) does seem like a discussion of whether arithmetical propositions can be incorrigible and synthetic, or, as he says, whether or not they "depend on the nature of reality" (210, 212, etc.).

Of course, it is not a good reply that a question is begged against Kant. Kant has been dead for close to 160 years and science, as well as philosophy, has changed tremendously since then. The point is that there are good reasons not to identify corrigibility with being descriptive of the world in the sense of "depending on the nature of reality". Just to mention two cases, we have propositions like (1) and (2) below which somewhat resist such an identification:

(1) One ought legally (i.e., according to the law) to drive on the right hand side of the road.

(2) I have a mild eye-ache.

Proposition (1) is corrigible, but not descriptive of the world; (2) is both incorrigible and descriptive of the world.

When a person says it is illegal not to drive on the right, which is the same as (1), he could be led to withdraw his statement by finding out that the existing law requires driving on the left. Many facts enter into the truth of a proposition like (1); that there is an authority capable of legislating on such matters, that there is a procedure which has in reality been followed, etc. But (1) does not inform us of that, except in the sense that its truth *presupposes* those facts. (1) is a statement about legal obligation, whose simple denial is not the disjunction of all the statements denying those facts, but the proposition that it is not illegal not to drive on the right.

Gasking's own fellow-Wittgensteinians have been arguing that statements like (2) are not descriptive, just because they are incorrigible. They want to take them as highly developed signs of pain; but they are too well acquainted with human nature to deny that utterances like (2) can be true or false. Or when they want to deny that (2) can be descriptive or formulate knowledge and, hence, withhold the use of the adjectives 'true' and 'false' from (2), they have to fall back on "the *sincere* (or *honest*) utterance of . . . ". Obviously, statements like (2) do not fail to be descriptive of the world in the sense that their truth "depends on no fact about the world, other than some fact about the usage of symbols" (217). Furthermore, it will never do to accept that other persons can describe my states of pain because they see and hear me, whereas *I* who feel the pain cannot describe them. Surely, (2) may and does in fact function as a sign of pain to my hearers; but there is no reason to suppose that because (2) functions as surrogate pain behaviour *as well* it no longer serves to describe a mental state of myself.

At any rate, (2) shows that Gasking's assimilation of incorrigibility to uninformativity is far from obvious.

3. *Arithmetic and Life.* Gasking's final argument for the view that mathematical propositions are not descriptive of the world and depend on the usage of symbols hinges entirely on his claim that "we could use *any* mathematical *rules* we liked, and still get on perfectly well in the business of life" (213, my italics in 'rules'). His argument is roughly divided into two parts to show that:

(F.1) We could use any mathematics and compensate for deviations from the present one by means of an adequate technique of counting or measuring (213 ff);

(F.2) We could use any mathematics different from the present one and modify our physics (215-219).

3.1. *Rules.* Before examining the details of Gasking's argument it is important to note that he first speaks of "mathematical rules" and then of "mathematical propositions", as if the two expressions were synonymous. But, although we may say that rules are a species of proposition, not all propositions could be said to be rules. On the other hand, it is natural to say that a rule is not descriptive, but prescriptive; so that Gasking could at least *prima facie* be accused of begging the question.

Indeed, the typical propositions or statements of which we can say that we can use different ones and yet get on in the business of life are *rules,* particularly rules like

(1) It is unlawful to drive on the left-hand side of the road. We could certainly have a different law, making it unlawful to drive on the right, and life might be just as pleasant (or unpleasant) as it now is. However, it is not clear that arithmetical propositions like "$7 \times 5 = 35$" or "$7 + 5 = 12$" are rules. There is not a single bit of ground for saying that these

are rules governing the describing of the world in the way in which (1) is a rule governing the traffic of vehicles on the road.

For one thing, rules tell us what to do or what to avoid doing, but if a storm fells 7 trees and then 5 trees it has felled 12 trees regardless of whether there are human beings or not. No action is prescribed to anybody by the proposition "7 trees and 5 different trees are 12 trees".

Secondly, rules are formulated in sentences containing a "rulish" or prescriptive term like 'ought', 'forbidden,' 'required', 'illegal', 'unlawful', 'permitted', etc. It is not easy to find a place for such terms in "$7 + 5 = 12$".

There are indeed mathematical rules like "To multiply a whole number by 10^n you may (are permitted to) write n zeros to the right of that number". There are also rules for the application of mathematics like "To find out how many tiles, each 1 foot square, you need to pave a room 20×50 feet, you ought to multiply 20×50". But the important thing is that all such rules are derived from mathematical propositions asserting a given relationship between numbers. The rules are, in Kant's terminology, hypothetical imperatives; they follow from the non-rulish propositions, by means of the ordinary logic of rules or norms[2]. Similarly, once we have the proposition "$7 + 5 = 12$" we have the ground for the rule "If you are discussing the number which is the sum of 5 and 7, you may discuss instead the number 12", or a ground for the rule "If you put 7 apples in the basket and then 5, you may say that you put 12".

In any case, it is worth noticing that rules, i.e., real, ordinary rules like "It is not permissible to drive on the left", may be said to be:

i) corrigible — the legislators might not have enacted such a law;
ii) synthetic — in the sense that they could be disobeyed;
iii) not empirical — they are not discovered by observation;
iv) not descriptive of the world — but prescriptive;
v) not dependent on the usage of symbols only — enactment is necessary;
vi) replaceable — i.e., using different, incompatible rules could get along in the world.

It is not difficult to see that Gasking's view of mathematical propositions as rules lets him hold that mathematical propositions are replaceable (feature vi), not empirical (feature iii), and not descriptive of the world (feature iv). One can see that the underlying movement of Gasking's argument takes him from the incorrigibility of mathematical statements to their non-empirical character, and then, since he assimilates non-empirical to non-descriptive (pp. 206 f., 219 bottom), the dichotomy "either descriptive or rulish" forces him to classify them as rules.

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[2] Cf., e.g., H. N. Castaneda, "Nota sobre la lògica de los fines y medios", *Universidad de San Carlos* (Guatemala), No. XXIX (1956); "Imperative Inference", *Philosophy and Phenomenological Research (forthcoming)*.

However, once we reflect on the nature of rules it is clear that Gasking is now heading for trouble. At the bottom of the replaceability of ordinary rules lies their corrigibility. Surely, to say that the rule may be to drive on the left rather than the right is in part to say that it might have been *false* that we ought to drive on the right. Likewise, the possibility of a queer mathematics in which "$7 \times 5 = 75$" entails that "$7 \times 5 = 35$" *could* be false, i.e., that it is corrigible. Therefore, Gasking's argument has the perplexing characteristic that, even though he identifies corrigibility with being descriptive of the world, his strongest step involves showing that ordinary arithmetical propositions could be falsified *and so* are not descriptive of the world!

Clearly, a proposition whose truth depends on linguistic use alone, like "Bachelors are unmarried", cannot be falsified *at all.* We could drop the word 'bachelor' and use 'solor' in its stead, but the proposition "Solors are unmarried" is exactly the same, and continues to be unfalsifiable. We may stop using words with which we could say that bachelors are unmarried, but that is not to use a queer system of family relationships.

I do not know of any way of repairing Gasking's argument. If mathematical propositions were rules he could defend their replaceability, without having to bother with whether they are corrigible or not. But I have indicated some reasons which make it difficult to disguise them as rules. I pass over the view that analytic propositions are rules and that, since mathematical propositions are analytic, they must be rules. This view begs the very question we are discussing: whether or not mathematical propositions are descriptive of the world, i.e., non-analytic[3]. However, a view can be illuminating even though it is false and the arguments for it are invalid.

3.2. *Queer Countings and Measurings.* Gasking argues that we could employ a queer arithmetic in which $6 \times 4 = 12$ and get on perfectly well with the business of life. He does not define a queer arithmetic, but it is clear from his discussion that he does *not* mean one or more changes of labels. In other words, to use the formula "$6 \times 4 = 12$" is not merely to use '6' to mean the number 3, and perhaps '3' or 'w' to mean the number 6. These are trivial changes in numerical terminology. Obviously, such changes are present in our daily experience, as when we translate from English into German, or as when a person translates numbers into the binary system to make a computer work some problem. By a 'queer arithmetic' Gasking means something more exciting. Presumably, we are to think of queer arithmetics on the analogy with non-Euclidean geometries. Provisionally, just to fix the sense of the argument, we may say that a *queer arithmetic* is a system of propositions about natural numbers in which we have a different multiplication table. (This topic will be given an independent and detailed discussion in Part II of this paper.)

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[3] Besides, the major premise is false. For a thorough discussion of the view that analytic propositions are rules, *cf.* A. Pap, *Semantics and Necessary Truth,* (New Haven: Yale University Press, 1958), 170 ff, 182-5.

Gasking says that in that queer arithmetic the product $m \times n$ is the number that we obtain by the formula: $(m+2)(n+2)/4$. (Let us not ask now about the nature of the translation in question.) He argues that by means of a different "technique of measuring lengths" (214) a builder could pave a room 6 yards long and 4 yards wide:

> "He lays his yardstick along the longer side, with the '0' mark of the yardstick in the corner, and the other end of the stick, marked '36 inches', some distance along . . . he counts 'one'. He then pivots the yardstick on the 36 inches mark, and swings it round through two right angles, till it is once more lying along the side of the room — this time with the '36 inches' mark nearer to the corner . . . he counts 'two' . . . he pivots the stick about its middle and swings it round . . . he counts 'three'. He then swings the stick round through two right angles, pivoting on the '36' mark, counting 'four'. He then reverses its direction, as before, counting 'five' . . . " (213 f.)

Clearly, if he were to count the tiles, after he paved the floor, the builder (hereafter to be called "Gaskon") would have to count: 'one' for the first tile, 'two-three' for the second, 'four' for the third if it is the last, or 'four-five' if it is not the last, etc. Hence, the "queer technique" of measuring requires a similarly "queer technique" of counting. (This, incidentally, shows that measuring and counting are not independent concepts or operations, as Gasking implies on p. 215.) Counting is, in fact, the simplest form of measuring, and every measuring includes it or presupposes it.

It is important to note that Gasking's talk of a "different technique" is deceptive. He is dealing with a different concept of counting and measuring. Different techniques under the same ordinary concept of counting are, e.g., 1) counting by saying 'one' 'two', 'three', etc. to different objects, 2) counting by pairs or fives, 3) weighing the totality of objects, weighing one of them, and dividing the two numbers (when the objects involved are supposed to have the same weight), etc.

In Part II, I shall show that "queer" countings either lead to contradictions or involve at most an infinity of trivial changes in numerical terminology. In the present case we may notice in passing that Gaskon is supposed to count twelve tiles as we do, in order to pave the floor. But once he has paved the room he will *have to* count in the queer way he measures to speak of 4 and 6 tiles or squares on each side, as illustrated:

	1	2–3	4–5	6
1	1	2–3	4–5	6–7
2–3	8–9	10–11	12–13	14–15
4	16–17	18–19	20–21	22

$6 \times 4 = 12$

Gaskon must count abnormally to find that the room is 6 yards long and 4 yards wide; but then he will find that he needs 22 tiles, whereas, according to his arithmetic, he needs only 12.

We should also notice that in such a queer arithmetic, in which we are supposed to have all natural numbers, we cannot employ the usual rules of addition: "2 tiles and 1 tile = 4 tiles"; on the other hand, " 3 tiles and 1 tile" has no meaning. But let us postpone the development of these matters.

One can easily see that Gasking's argument looks persuasive only because he has isolated 1) the measuring of the room from the counting of the tiles or squares, and 2) multiplication from the remaining body of arithmetic. One suspects that, just as it is impossible to isolate measuring from counting, it is not possible to isolate both counting and measuring from the properties of physical objects. That is to say, all the cases that Gasking discusses are probably in the end instances of his last case: that, by changing the laws of physics, we might very well get on in the business of life with a queer arithmetic. Thus, his last argument is the fundamental one.

3.3. *Queer Physics.* Gasking's more dramatic claim is that Gaskon could count and measure the way we do, but multiply according to different tables, and yet get exactly the right number of tiles to pave his room (217). He offers both a general argument and an illustration. I quote the former in full:

> "Imagine that we did use a 'queer' arithmetic, in which $3 \times 4 = 24$ . . . if we did use this queer system, we should stick to the proposition '$3 \times 4 = 24$' no matter *what* happened, and ascribe any failure of prediction of builders' requirements, and so on, *always* to a mistaken view of the physical laws that apply to the world, and *never* to the untruth of the formula '$3 \times 4 = 24$'. This latter proposition, if it *were* part of our mathematical system, would be *incorrigible,* exactly as '$3 \times 4 = 12$' is to us now." (217.)

He then goes on to illustrate. When he sees that 12 tiles are enough to pave the room, Gaskon would say, e.g., that "the tiles must expand to double their area on being put into the room" (218).

Of course, Gasking is right in everything he says here. Since mathematical propositions are incorrigible, *if* "Napoleon weighed 50 pounds at birth" were a mathematical proposition it would be incorrigible. This contrary-to-fact conditional is logically true. But from it we can infer neither (a) that "Napoleon weighed 50 pounds at birth" is a mathematical proposition, nor (b) that it is incorrigible, nor (c) that it is a legitimate candidate as a mathematical truth. Gasking's argument is simply an *ignoratio elenchi.* The issue is neither whether "$3 \times 4 = 24$" would be incorrigible, nor what we would say if certain facts did not come out right when we used that formula for prediction. The issue is whether the formula is a legitimate alternative in arithmetic — in the way in which the proposition in Riemannian geometry that there are no parallels is or is supposed to be a legitimate alternative to Euclid's postulate.

This argument also brings into focus the perplexing feature mentioned in section 3.1. If "$3 \times 4 = 12$" is absolutely incorrigible, and this is the assumption from which Gasking is arguing, *nothing at all* could make it false, regardless of how much the description of the facts were to be changed. The only possibility would be, as was the case with the analytic proposition "bachelors are unmarried", to make a trivial terminological change—but this has been excluded by the definition of a queer arithmetic.

Even Gasking's illustration looks more plausible thanks to his method of isolating propositions and cases. For us to say meaningfully that the tiles double in size when brought into the room, they must be smaller outside the room; moreover, things inside the room must have the same constant size, everything else being equal. As Gasking correctly emphasizes:

> "In this case we find that the tiles expand by *measuring, counting and calculating in a certain way* — by finding that the tiles each measure 1×1, that the room measures 3×4, or 24 square yards, and that we can only get 12 tiles in it. This is our sole *criterion* for saying that the tiles expand. That the tiles expand *follows from* our queer arithmetic." (219; his italics.)

Let us suppose now that Gaskon draws a rectangle like the floor of the room outside the room and tries to pave it with the same tiles:

	1	2	3	4
1	1	2	3	4
2	5	6	7	8
3	9	10	11	12

$3 \times 4 = 24$; yet 12 tiles are enough

Then, inside the room, he cuts a piece of paper into three strips and each strip into four pieces. According to his "multiplication" table he should have 24. He counts the pieces of paper and finds only 12. Thus, it seems that Gaskon should say that things expand: (a) when they are brought into the room, (b) when they are taken out of the room, (c) when they are left in the room, and (d) when they are left outside the room. In other words, there is no expansion: from what size did they expand? So Gaskon can say that from his queer arithmetic it follows that the tiles *cannot* expand.

But now Gaskon has become possessed of the scientific demon. He counts 12 tiles, then he makes 4 piles of three each, so that according to his queer arithmetical proposition "$3 \times 4 = 24$" he has to say that they have doubled. He counts "one, two, three; four, five six; seven, eight, nine; ten, eleven, and twelve", so that the additional ones vanish; but since the piles have remained intact he has both 12 and 24 tiles — yet 12 and 24 are different numbers!

Gasking may say that there is no contradiction, for there are two different operations involved, and if we distinguish them everything is all right. Well and good; but then Gaskon is bound to say that the operation ‘×’ cannot be an abbreviation for counting, and that the only way of determining how many tiles he needs to pave the room is to measure the floor by counting the squares, as in the diagram above. He may eventually invent ordinary multiplication as a shorthand for addition. In other words, the operation Gasking represents by ‘×’ is *either* the source of contradictions if both it and ordinary multiplication are taken to be the same operation, *or* it is an altogether different operation which can peacefully coexist with ordinary multiplication. In the latter case Gasking is not discussing a queer arithmetic, but only ordinary arithmetical operations which (i) are not shortcuts for counting, (ii) are correctly performed on numbers without contradiction, (iii) are not involved in counting and (iv) are such that the propositions formulating them are *already* incorrigible, like any other arithmetical proposition.

In other words, if Gaskon’s ‘queer’ arithmetic involves no contradiction, it is only a terminologically different arithmetic, in which the operation $\neq$, defined by “$m \neq n = 2\,(m \times n)$”, has been called “multiplication” and the sign ‘×’ has been used to supplant ‘$\neq$’—in spite of the resulting ambiguity!

I conclude, therefore, that Gasking has not proved his most important claims, (B) that ordinary arithmetical propositions are not descriptive of the world, (C) that they depend on linguistic usage only and (F) that queer arithmetics could very well be applied to reality. I have pointed out a conflict between his theses (B), (C) and (F). So perhaps one of them may after all be true. Even though I have attacked Gasking’s arguments for (F), it is not completely clear from my discussion that (B) and (C) are true. For one thing, all three may very well be false. In any case, I propose to examine thesis (F) on its own merits.

II

Alternative Arithmetics

1. *Queer Arithmetics.* Let us follow Gasking in the use of the term ‘queer arithmetic’. But we must give it a precise meaning if we are to get somewhere. The following definition seems to me to be adequate for a general discussion of Gasking’s problem:

Def. A *queer arithmetic* is a system of propositions about numbers which includes at least one proposition incompatible with the propositions proven in arithmetical treatises and manuals.

It should be noticed that:

a) Peano's postulates alone do not constitute a queer arithmetic, but only an incomplete formulation of arithmetic. Those postulates (to be discussed below) formulate formal properties of the sequence of natural numbers, but do not tell us how numbers are used in counting and measuring.

b) There are trivial changes that can be introduced in the numerals, which cannot produce a queer arithmetic. One such trivial change would be to use the sign '4' to stand for the number 3, and either the sign '3' or another sign to stand for the number 4. Clearly an infinite collection of such trivial changes does not yield a queer arithmetic. We may speak of a *trivially different arithmetic*. One such example would be to use the signs in the first row for those just underneath them:

(G1) 1, 2, 4, 6, 8, 10, 12, 14, . . . , 2n–2, . . .
1, 2, 3, 4, 5, 6, 7, 8, . . . , n , . . .

(G1) is the trivially different arithmetic involved in Gaskon's queer countings and measurings discussed in Section I.3.2 above.

c) There are (as we have seen) trivial changes in the terminology of arithmetical operations or functions. We may call "multiplication" the operation of doubling the product of two numbers, and we may speak of "semi-multiplication" for ordinary multiplication. Again, these changes may be compounded to infinity. In an excess of clarity we may call the systems obtained by such changes *trivially changed arithmetics*. One such arithmetic, we saw in I.3.3. above, misled Gasking.

2. *Ordinary Arithmetic*. The ordinary arithmetic of natural numbers can be characterized by two sets of propositions:

A) *Peano's postulates*, which determine the natural sequence of numbers:

(P1) 1 is a number.
(P2) Every number *n* has an immediate successor, to be called *n'*, which is also a number.
(P3) If two successors *m'* and *n'* are the same number, then *m* and *n* are the same number.
(P4) 1 is not the successor of any number.

(P5) Every property P is possessed by all numbers if 1 has P and if, when *n* has it, *n'* has it too.

B) *The principles of counting,* which formulate the basic manner in which natural numbers function in experience:

(C1) To count the objects of a collection or aggregate K is to establish a one-to-one correspondence between the objects of K and a set N of numbers (or numerals), such that:

(C2) N includes 1;

(C3) there is at most one number in N whose immediate successor is not in N;

(C4) the number of objects in K is the number mentioned in (C3) if it exists; otherwise the number of objects in K is infinite;

(C5) the one-to-one correspondence may be carried out (i) by actually attaching one number (or numeral) to each object; or (ii) by forming partial non-overlapping correspondences of type (i), as when we count by twos or fives or hundreds; or (iii) by specifying a rule for actually attaching numerals to as many objects as we please.

Obviously, since, as Peano, Dedekind, etc. have shown, all formal properties of numbers follow from (P1)–(P5), a queer arithmetic worth discussing must be one which differs from ordinary arithmetic in at least one of Peano's axioms or one of the principles of counting. Otherwise, the different arithmetic is trivially different or trivially changed, or self-contradictory. Thus, there are at most ten fundamental types of queer arithmetics.

3. *Numbers and the World.* I want to argue in some detail that Gasking's claim that a queer arithmetic could be applied to describe reality is ungrounded. Every queer arithmetic, i.e., one which does not turn out to be either an incomplete or a trivially different or changed arithmetic, is inconsistent or inadequate to help us get on in the world in all situations.

3.1. *Non-Peanian Arithmetics.* To begin with, I shall show that a queer arithmetic which contains a change in Peano's postulates requires a different concept of counting. So if, as in Gasking's final argument, we are to suppose that Gaskon has a queer arithmetic, but his counting and measuring proceed as usual, then Gaskon's computations must produce results contradictory to those which he obtains from counting.

CASE 1. If 1 is not a number we must start counting with two. Thus, when Gaskon says that he needs three tiles he actually needs two, and in general whenever he says "n objects" we can translate "n–1 objects'. Hence, (P1) cannot be the source of a queer arithmetic.

CASE 2. If we abandon (P2), at least some numbers, say 8 and 25, do not have immediate successors, or if they do their successors are not numbers. In the former situation we could not count beyond 8, for (C3) does not allow us to make a new start; aggregates with 10 and 12 objects would be uncountable. This may not be inconsistent, but will certainly make it impossible for Gaskon to pave his room — except by a lucky chance, since he could not have a way of distinguishing 12 tiles from 11 or 14. Of course, this is not the queer arithmetic Gaskon was left with, for he was supposed to know that 11 apples and 12 tiles were exactly what they are for us; he was able to count up to any number. Thus, Gaskon was supposed to be able to do the impossible.

On the other hand, if the sequence of natural numbers is only supposed to have, as it were, a hole, we find a trivially different arithmetic. If 8 has a successor, 9, which is not a number, when we count we are just to bypass 9. Arithmetic is the same as usual, except that after 8 (and before 26) all numbers are moved one place, and after 26 they are moved two places.

CASE 3. If 1 is the successor of, say, 25, (C1) and (C5) require that we stop counting at 25. Thus, abandoning (P3) takes us back to Case 2.

CASE 4. Suppose that 100 and 50 have the same successor, even though they are different. Thus, we may have the following sequence of natural numbers:

$$\begin{tikzcd}
1, 2, \ldots, 50 \arrow[dr, dash] & \\
& 51, 52, 53 \ldots \\
a, \ldots, 100 \arrow[ur, dash] &
\end{tikzcd}$$

According to (C2) we must count "One, two, . . . "; according to (C3) we must say "fifty-one" after saying "fifty", and cannot use "a".

Thus, the segment "a, . . . , 100" is altogether irrelevant, and we obtain the usual arithmetic.

On the other hand, suppose the resulting sequence were:
1, 2, . . . , 50, 51, 52, . . . , 98, 99, 100, 51, . . .
If the sequence repeats in cycles after 50, according to (C1) and (C5) we cannot count beyond 100, and are back to Case 2. If the numbers following the second 51 are different, say, 101, 102, . . . , we are led to contradictions because of the ambiguity of "51 apples". If one 51 is disregarded in counting we obtain the normal arithmetic.

CASE 5. Suppose that there is a property, call it R, which 1 has and if n has it n' has it too, but not all numbers have it. If only (P5) is false we get a transfinite arithmetic, but transfinite numbers are not used in counting, so our ordinary arithmetic is not queer. If no other numbers, transfinite or appendetic like "a, . . . , 100" at the beginning of Case 4, are included, then the negation of (P5) produces inconsistency; for nothing could fail to have R. If besides (P5) (P4) is also false, then a situation like the one described above with the segment "a, . . . , 100" preceding 51 would be possible. But this does not produce a queer arithmetic, only a "larger" arithmetic which includes ordinary arithmetic.

3.2. *Queer Countings.* Suppose now that a queer arithmetic includes both Peano's axioms and different principles of counting. Again, there are at most five basic types of queer arithmetic. I shall show that they either involve self-contradiction or prevent Gaskon from getting on in the business of life.

CASE 1. We give up the one-to-one correspondence, for example:

a) we count "one-two, three-four, five-six, . . . ",
b) we count "one, two-three, four-five-six".
c) Gasking's special technique of counting discussed in 3.2. of Part I: "One" for the first object; we say "two" for the second if it is the last, otherwise "two-three"; we say next "four" or "four-five", depending on whether the object is the last to be counted; and so on.

In some of these "countings" we just use a term like 'one-two' for '1', 'three-four' for '2', etc., so that there is no queer arithmetic, but only a trivially different one. At any rate if we are to avoid contradictions we cannot, e.g., allow that 12 apples for *us* be regarded both as 25 and as 30 apples and say that 25 and 30 are different numbers. We must have a general rule as to how the correspondence between numerals and objects is to be made. Thus, using a queer arithmetic of this sort, we should speak of $\phi(n)$ objects for every situation in which we now say that there are *n* objects. And the counting will have to be "$\phi(1)$, $\phi(2)$, $\phi(3)$, . . . , $\phi(n)$". Now, if the other numbers are disregarded we just have a trivial change in terminology; if we consider that Peano's postulates apply to the ϕ-numbers as well as to the other numbers, we do not produce a queer arithmetic, either. This only shows the well-known fact that Peano's postulates are not a complete or unique characterization of the sequence of natural numbers. Since the natural numbers are those attributed to objects in counting, the ϕ-numbers *are* the natural numbers under a new name.

Let us consider now the correlation of one numeral to several objects. In fact we often do it, as, e.g., when we count pairs or triplets or piles. But suppose we counted: "one (taking or pointing to one object), two (taking two objects), three (taking two), . . . ". We could not count the persons forming a monogamous marriage or the persons forming two such marriages. Clearly, the arithmetic is not queer, but we may find it difficult to determine how many tiles we have to buy for a certain room whose dimensions we cannot ascertain, unless we interpolate "fractions" and come to use the trivially different sequence "1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, ... ".

CASE 2. If we never start counting with '1', we are only changing the names of the numbers.

CASE 3. If we are not to use certain numerals in counting, they have to be fixed by a rule; otherwise we may fall into contradictions, or provide no way of finding agreement, for we may count from 1 to 15 once and then count the same objects correctly, jumping, say, 8 or 10, and obtain 16 or more objects. Let the numerals to be omitted be fixed. Then we may just have a trivially different arithmetic with terminological changes after those numerals. But we may find that our arithmetic is inadequate or leads to contradiction. Suppose, e.g., that '9' is omitted. Eight apples and 1 apple will be 10 apples. If 8 and 2 are also 10, we get a contradiction; if 8 and 2 are 11 then we can use these addition tables in life, but we have only substituted '10' for '9', '11' for '10', etc.

CASE 4. We may abandon (C4) and take as the number of counted objects one whose successor is also mentioned in the counting operation. This amounts to correlating several objects to a numeral and leads us back to Case 1 above. If we take another number, then we are actually correlating several numerals to one object, which also fails to produce an applicable queer arithmetic.

CASE 5. Obviously contained in the previous cases.

3.3. *Conclusion.* Queer arithmetics in the sense defined cannot apply to the world. Thus, what I have called Gasking's thesis (F) is false, namely, that we are free to use a queer arithmetic provided we introduce certain changes in our technique of counting and measuring or in our description of the facts, i.e., our physics.

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TRUTH AS THE ACHIEVEMENT OF KNOWLEDGE

By Roy Wood Sellars

In this paper I want to give, in outline, what seems to me to be the correct framework for handling the categories of truth and knowledge. I call these epistemological categories. This is in accordance with my contribution to the book, *Essays in Critical Realism* (1920). I shall hold that knowledge is the primary category and that truth is its achievement through the operation of verification. In this context, I speak of beliefs as knowledge-claims. And, as I see it, knowledge has objective import and is concerned with the constitution of *being.* In this fashion, epistemology links up with ontology. Thus, I am a physical realist.

The two categories, knowledge and truth, are very intimately related; and it is because they are so intimately related that confusion occurs. We can put it this way: Knowledge, as an achievement, points outward to its object, while truth points not only to knowledge but to its complex foundation. That is where verification comes in.

I have in mind here, of course, empirical knowledge as it begins at the elementary level of perceiving and advances in everyday life and in science. Perceiving which achieves perceptual knowledge is called veridical. It tells us something, *in a guided way,* about the world around us. I speak here of direct, realistic empiricism and stress guided reference tied up with the pole of response. The functional unit is S-C-R. I break completely with the pre-biological unit of traditional empiricism, S connected with a sensation.

But I must not anticipate too much. To come back to the epistemological categories, I would take the adjective "true" to apply to statements taken as knowledge-claims. So applied, it means "is a case of knowledge and acceptable as such". This framework and terminology I worked out fairly early. It can be found in my first book, *Critical Realism* (1916), chapter 10. Knowledge, as an achievement, *implies* the correspondence, or adequacy, of the statement to its object. Correspondence is not a test but an implication. Here I find that my line of thought connects up with the semantic theory of truth in recent logic. But, as an epistemologist, I must explain this achievement by looking at the foundations of verification; which will take me to a consideration of the whole mechanism of perceiving as a controlled and guided activity of the organism in its world.

All the niceties of present usage can be employed. We can distinguish between sentences and statements and take a statement, as an assertion, to be an objective knowledge-claim. We must, however, take knowledge, as an achievement, to be an ultimate category which *cannot* be reduced but which can be explored. I myself take it to express the disclosing grasp which verified statements have on their referents, that is, broadly speaking, on things and events. It is usual to speak of such disclosures as facts. In knowledge, we have facts about things. And facts are *in re* only in the sense that knowledge is attained. It is here that epistemology links up with ontology. I am accustomed to speak of knowledge as a deciphering of the constitution of being. It is a remarkable achievement with a complex foundation which goes back ultimately to the controlled functioning of the sense-organs.

I shall have something more to say about knowledge in the sequel, when I talk about existence. For the realistic empiricist, knowledge is about things and not about ideas. Thus things are cognitively attained. Hence we can explore knowledge without seeking to reduce it so something else. While *sui generis* as an achievement, the whole machinery of sense organs and cortex, of sensations and concepts, is at its service. We can pass from the *what* to the *how*.

A few historical remarks are in order. American realism reflected dissatisfaction with both idealism and pragmatism. The expression "epistemological monism" was coined by the new realists to characterize their searchlight, or presentational, view of perceiving. As Perry put it, the idea *is* the thing. I coined the expression "epistemological dualism" to mark the rejection of this view of knowing. I held that knowing is an affair of directed reference and characterization. The reference is built up on the response pole of perceiving and carries conceptual characterization with it. It is dualistic in the sense that it rejects the presentationalism of the searchlight and the sensationalistic tradition. It stresses knowing *through* ideas, or statements, referentially directed. It is an affair of aboutness and of conceptual application. But it is a form of direct realism as against Locke and Kant. I would also remark that it concerns itself with physical things and is thus existential in import. This is the reason why to say that this thing before me exists is tautologous. Russell's use of the term "exists" as concerned with the applicability of concepts is, of course, secondary.

Since knowledge is achieved through statements, it follows that these must be *such that* they can convey knowledge. This is their correspondence or adequacy. But it must be tested and verified in operative ways going back to the foundations as well as to its working. As we have said, correspondence is not itself a test but an implication of the fact of knowledge. I brought this point out in both the *Philosophy of Physical Realism* and in my study of Realism in Winn's *American Philosophy.* As I see it, the semantic definition of truth to the effect that the statement, "It is raining" is true if, and only if, it is raining is an application of the above analysis. What more is needed is the understanding of tests and foundations. And that is more an affair of epistemology than of logic.

One more introductory point. I have spoken of S-C-R as the functional unit. In man, through language, C has a tremendous development so that response is lifted to communication and designation. Yet it is still the underlying mechanism. I am interested in seeing how linguists and psychologists will work this out. The book entitled *Language, Thought and Culture,* edited by Paul Henle, has interesting material in this connection.

And now I am going back to the first decades of this century when there was something of a stalemate between three classic conceptions of truth, namely, the coherence conception of idealism, which goes back at least to Spinoza; the pragmatic conception, which stressed verification through predictive working, and the correspondence theory, which held that ideas, if true, corresponded to something objective. I leave out the Cartesian appeal to self-evidence, as associated more with intuitional rationalism than with empiricism.

These positions had their setting and their points. Coherence emphasized consistency and inclusiveness and rejected the realistic dimension. Royce should be read in this connection. Both Bradley and Bosanquet were experientialists. Pragmatism was strong in its emphasis upon the temporal process of verification and working but did not dwell upon the import of knowledge-claims. It at least tended to lack the realistic dimension and did not do much with perception. To me it was vague about physical things and such entities as atoms and electrons. If in the James form it was too subjective, it was made more objective by Peirce and Dewey. Here the emphasis was upon the instrumentality of ideas and their acceptance, in the end, as affairs of competent decision. The correspondence tradition remained in the background as supposedly outmoded. Russell urged it but in a varying epistemological setting. As with the later positivists, sense-data stood out as primary. I have a pretty clear memory of the issues of this period. Both idealists and pragmatists rejected correspondence because they could not see how it could be tested. Certainly, it could not be done by comparison.

It was clear that something more basic had to be done. The realists tried to do this. I am going to expand the argument of the critical realists. I do not think it got a proper hearing. Soon there were too many excursions and alarms. There was a shift away from epistemology to proposals about meaning and language. These things have to work themselves out. I have the impression that positivism *qua* phenomenalism is fighting a losing battle. What is now needed is a clear, realistic alternative. I have mentioned the appearance of the semantic definition, which fits in with realism. The only other novelty seems to be the performatory theory of truth which calls attention to various linguistic uses of the word "true", such as that which is equivalent to a nod of agreement. But it neglects verification demands, as J. Kincade points out in the July 1958 number of *Mind*.

It will be remembered that it was the feeling of both the new realists and the critical realists that neither the idealists nor the pragmatists had done justice to the realistic demands of perception. I have the impression that when Dewey moved to Columbia he was somewhat affected by Woodbridge and Montague, both of whom were realists. The older Columbia men could, undoubtedly, give us information about this point.

Now I am not going to say anything much about the new realism, except by way of contrast. The sceptical dictum of the early thirties, as I recall it, was to the effect that the new realists could not account for error and the critical realists could not account for truth. It is to the second half of this dictum that I want to address myself.

In the first place, critical realism was *not* a revival of Lockianism nor of Kantianism, with its things-in-themselves. Rather, it expressed a re-analysis of perceiving, with emphasis upon the pole of response, so that the functional unit was S-R, or better, at the human level, S-C-R; that is, with learning and concepts intercalated between stimulus and response. In the old tradition of the "causal theory of perception", the unit had been the stimulus with its effect, sensation. The more complete biological unit was ignored because it was the age of classical physics and Cartesian dualism. As a consequence of the stimulus, sensations popped up in the soul-mind. Positivists and phenomenalists have just gone one better than this by starting with sense-data. To many young men, it sounds hard-boiled. I have always held that a frank materialism is still more hard-boiled.

Now the new realists were aware of the above shift of framework in the direction of response and behaviourism. I like always to give my opponents their due. Dewey became increasingly behaviouristic as the years went by. But, as I see it, the new realists jumped too hastily into the pan-objectivism of extreme behaviourism with its rejection of consciousness and the subjective. Bergson's view of the brain seems here to have influenced Perry. And it always seemed to me that Dewey fought shy of consciousness and the subjective, handling them in a sort of dialectical fashion. I had the impression that he concentrated his attack on the new realism, especially on its universalization of cognition, and on the essence doctrine of Santayana.

The new realists were afraid of representationalism. Hence their gambit of the searchlight view of perceiving with its rejection of consciousness and the subjective. I, on the other hand, devoted my energies to a basic reconstruction of our ideas of perceiving. I regarded sensations, connected with the stimulus, as guiding perceiving and so emphasized the response pole of the whole activity. At the human level, learning and language intervene to make perceiving an affair of directed designation and characterization. Here is where objective reference gets its base of operations. It is that chair over there which I am looking at and characterizing. I am quite ready to admit all the modifications of atomistic sensationalism needed and to stress constancy and *Gestalten*. But the point is that perceiving is a directed affair and concerns itself with the things around us. Visual perceiving is guided by my visual field but is concerned with what I am looking at.

With my friends, the animal psychologists, I like to trace the development of this framework and see how it flowers at the human level in designation and characterization and in the rise of *categorial meanings,* such as existence, thinghood and causality. Hume could never account for them on his truncated sensationalism and Kant had to introduce them *a priori.* I sketched this handling of the categories in my *Evolutionary Naturalism* (1922).

Now it is obvious that this line of analysis of perceiving confronted the mind-body problem on different terms than did either the new realism or Dewey. It had to face up to it. I leave Montague's twistings and turnings out of the picture. He was less doctrinaire than Perry; and quite aware of the complexities of the problem. But I do not think he had the right epistemological lead.

I developed a double-knowledge approach in which I held guiding sensations to be "natural isolates", in cerebral processes, which could be experienced participatively, while the neurologist could have about the cerebral event only the sort of knowledge which takes its rise from referential perceiving. I do not think that Leibnitz's mill-machinery analogy does justice to this situation.

Sensations can be quite subjective and functional. But perceiving has its referential pole which is objective in import and is enriched by gesture and language. It is at this response-pole that realistic empiricism escapes from the subjectivism which haunted it from its inception and still lingers in phenomenalism. We perceive, refer to, and characterize the things around us. It is in this referential framework that concepts develop and operate. They are tied in with symbols and their use. Here we speak of understanding the meaning of words. There is no doubt but that Wittgenstein has made contributions here.

Perceiving, so taken, is concerned with a common, objective world around which communication can develop. I can point understandingly to the same thing you are pointing to. We are parts of a world and agents in it. The Cartesian gambit fails. Those who, like Moore, start with sense-data, are still trying to prove an external world. I take this to be an artificial problem due to a wrong start.

We are now ready to come back to the question of knowledge and truth. It should be clear that, in this framework, directed knowledge-claims are primary. We have good reason to believe that we are perceiving, that is, denoting and characterizing the very things we are adjusting ourselves to. We do not first know ideas and then *infer* things. We have undercut vicious representationalism. In this context, "true" can be taken to mean "is a case of knowledge". The knowledge-claim is primary. And this signifies the acceptance of *facts* about objects. These facts are the sort of disclosures knowledge can give. They are cases of cognitive attainment. As I have indicated, we can explore knowledge but not reduce it to something else. But we are aware that it is conveyed by statements having a conceptual composition. If knowledge, then the statement must be *such that* it can do the job. Thus correspondence, or adequacy, is an implication of the fact of knowledge.

But statements must be subject to test before they are acceptable as giving knowledge. Here is where the category of truth enters. It is Janus-faced and points to knowledge by way of verification. And this can be only by way of tests. And reflections on these lead us to foundations in epistemology. Empirical statements must do justice to observations. True; but how is it that observations have this cognitive value? There must be objective control and some kind of communication to give them this cognitive value?

First things first. We pass from knowledge as an achievement, and what it implies, to truth and its tests, and from tests to their foundation. When we ask ourselves why we think we have knowledge about our world, we are likely to reply that our knowledge presents us with an amazing and detailed picture which has slowly been achieved and that it also enables us to predict and control. It follows that our tests have done their job. Taking perceiving as our paradigm, we note how we are guided by sensory data and how these are relevant to the application of our concepts. Here we have a sort of empirical if-then relation which is *a priori* in C. I. Lewis's sense and which, surely, throws light on the growth of the concepts. This situation does not seem to me to have been fully investigated as yet. However, I do think it to be the case that we can attain empirical certainty at the common-sense level. If so, according to my argument, our verified statements correspond to their objects. They could not give knowledge otherwise.

But we still have the "sixty-four thousand dollar" question. As Heisenberg points out in *Physics and Philosophy,* the most recondite theories play into observational data. The epistemologist must have some theory to account for the "cognitive value" of these. Since we do have knowledge about our world, and the statements mediating such knowledge are checked logically and observationally, the data must transmit to us something about their causes; they must be in responsible dependence to the objects we are helped to perceive. Here we have a causal theory of sensory data and not a causal theory of perceiving. That is, we are concerned with a study of the stimulus pole of perceiving, with the how, or mechanisms involved, and not with perceiving, as a referential claim built around response.

Terms such as "communication", "reproduction of pattern", "appearing", have arisen in this connection. Techniques of communication are being applied to the sense-organs. TV has some relevance to vision. It is clear that, through the sense-organs, the organism is being alerted to features of the environment. It is not a passive affair. An active supplementation of exploring and deciphering proceeds. Clues are often enough. Neurologists like Herrick speak of neural analytic and synthetic processes. Concepts arise as hypotheses to draw out as much as possible of information.

The job of the philosopher, as I see it, is to help to give the proper perspective. So long as he did not have the correct view of perceiving as a terminative reference and characterization, he could not help to analyse the steps in the operation. He got confused and confused others.

To make a long story short, if sensory data are under the control of objects in such a way that these objects may be said to *appear* in them and if concepts are forged under such controls, the foundations of empirical knowledge are given a natural explanation. As I see it, the response of the organism helps to elicit information. Scientific technique has been added to biological technique. We interrogate nature.

We live in an age of electron microscopes and amplifiers. Even the Greeks were aware of the limitations of our sense-organs. Nevertheless, the point of departure must be a satisfactory theory of perceiving at the level of common sense. It is this that the critical realist has sought to work out.

I shall now try to sum up and emphasize perspective. I have tried to show that critical realism could account for truth. First of all, it did justice to perceiving as a directed operation terminating in reference and characterization. For its biological and psychological framework it took S-C-R as the functional unit. Thus it broke completely with the basis of traditional empiricism which had landed it in sensory subjectivism. Rather do sensations guide perceiving. Perceiving is objective in import since it is concerned with the things around us. This gives us a tidy linkage with ontology. Existence is not so much an attribute as a categorial recognition of the objective situation. That is why it is tautologous to say that this thing I perceive exists[1]. The new realism had good intentions but did not analyse perceiving carefully enough; and so fell back on a presentational *tour-de-force*. Critical realism is epistemologically dualistic only in the sense that it takes knowledge to be carried by statements. These must have a referential import which is prepared for by directed response which is lifted by language to designation. Knowledge, as an achievement, implies the correspondence, or adequacy, of the statement, a point recognized by the semantic theory of truth. However, correspondence is not a test; for no direct comparison between object and statement can be made. Verification is a complex operation involving logical and empirical tests. To complete its theory of truth and knowledge, epistemology must examine the reasons for the "cognitive value" of sensory data. This leads us back to biology and the role of the sense-organs. Theories of communication are here in order. But we must not forget that the organism is active and uses the sense-organs. The growth and role of concepts as factors in perceiving must be studied. In short, human perceiving is a very complex operation. The point I wish to make is that it is objective in import. That is why I call critical realism a form of realistic empiricism. Naïve realism reflects this import but has no idea of the nature and mechanism of knowing. How could it have?

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[1] Contrary to Leibnitz, I would say that the universe *just exists*, neither contingently nor necessarily. These modals are not here relevant.

All this represents a break with Cartesian dualism. Mind must be taken in a functional way. My double-knowledge approach seems to me the best bet. We can use both dispositional terms and occurrent ones. Because of a weakness in epistemology, Ryle seems to me to have oversimplified his revolt from Cartesian dualism. But it was a step in the right direction.

In conclusion I would like to make some comments on Wild and his neo-aristotelianism and on C. I. Lewis.

Wild is quite right in demanding a direct realism. It is the thing itself which we are perceiving. That is why knowledge has its ontological import. That is why categorial meanings are decipherments of the constitution of things. But it seems to me that Wild falls back on Aristotelian *forms* too readily. In accounting for the "cognitive value" of sensory data, I appealed to modern communication theory and the reproduction of pattern. But we must remember that the data must be taken up in a complex operation of guided response which flowers in reference and conceptual characterization. It is my belief that modern empiricism had to start with efficient causality, go through the fires of subjectivism, and then gradually work to a more adequate conception of perceiving. But I would like a *rapprochement* with the "Return to Reason" advocates. Concepts and reason play for me an important role, even in perception. Even in appraisal I am not an emotivist. But I do think that Wild and his group will have to face up to the mind-body problem more empirically.

As C. I. Lewis knows, our divergence lies in our theories of perception. He seemed to me to start from sensory presentations, pick concepts out of the air, deduce implications, and verify them. And he was persuaded that the critical realists were engaged in the fruitless quest of the copy-theory. It should be clear from the above analysis that I think of sensory factors as guiding a perceptual reference and characterization of direct, objective import. We do not first know ideas and then infer things which are like them. Now I welcome his appeal to concepts, which seem to be the instruments of knowing. But I should like to know more about their rise and testing. To me, the first stage of this seems to be in the perceptual situation itself. That Lewis starts from sensory presentations rather than from objective perceiving with its referential dimension seems to me to give it a phenomenalistic cast. But it is becoming apparent that Lewis is moving in the direction of realism, that he holds that meanings are applicable to denotables which are not sensory presentations. With this I heartily agree. It is my realistic empiricism. It may well be that we are nearer each other than seemed to be the case.

I do not think it is strange that an adequate base for theory of knowledge and truth has been hard to come by. The idealists built on subjectivism and were impatient of epistemology. As they saw correspondence, it was a self-defeating idea, as Blanshard still maintains. Only a Roycean God could directly test it. Coherence was, accordingly, the only alternative. I hope I have shown that testing works up to knowledge as an achievement, implying correspondence or adequacy. Perceiving is objective in reference and import but thought can work back to its framework and conditions. All of us realists felt that the pragmatists stressed verification and working, without doing justice to knowledge-claims. It was largely an attack on transcendental idealism. Thus an adequate theory of perceiving, which would do justice to both stimulus and response, remained a desideratum. By this reorientation I have sought to do away with the difficulty which, in the words of Bertrand Russell, "has troubled empiricism down to the present day"[2]. Sensations guide perceiving but are not its objects. Reference comes in at the response pole and is built up behaviourally and linguistically. Objectivity and interpersonal communication become operationally natural. Epistemology can thus link itself with ontology in a common-sense way. If I may judge from Quine, logicians may well welcome this kind of contact with "what there is". There is much analytic competence to-day. What it seems to need is a good starting-point.

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[2] See his "History of Modern Philosophy", p. 612 f.

NAÏVE REALISM AND ILLUSIONS OF REFRACTION

By M. Arthadeva

I

The facts of refraction provide the most serious difficulty for naïve realism. A stick appears to bend when put into water or, equally, appears to bend when, from a state of being immersed in water, a part of it is thrust out of the water: the part of the stick in the water does not form a straight line with the part that is out of the water. Why does the stick look bent when it is not? This is the way the question has been formulated. How is it known that the stick is not actually bent? It has traditionally been assumed that it is the sense of touch that proves this, that gives evidence of the fact that water is not, by itself, a sufficient cause for actually bending sticks. It has been said, and is still maintained, that, if we run our hands down the stick, we would feel that it runs straight: if the stick were actually bent we would feel the bend with our fingers. I do not think the sense of touch can establish this as easily as is supposed. We often know that a thing is bent by feeling it, because we bend our hand or our arm to touch the thing when the corner has been turned, or because of the sharpness of the corner itself; and we have learnt by previous observations, based mainly on sight, that such sensations occur when we feel along bent things. Such knowledge, however, is not the result of direct apprehensions and is liable, as direct apprehensions are not, to be mistaken. If a man felt the edge of a table that was circular and completely smooth with the same part of his finger all the time (this can be ensured by his walking round the table while he was feeling it or by the table revolving) he would not know whether what he was feeling was straight or circular. A man who observed his fingers feel along the refracted stick would see his hand turn the corner with the stick: consequently he may, and sometimes people do, get the illusion that the stick itself feels bent.

How, then, do we know that the stick is not actually bent? We cannot know this for a certainty on the basis of feeling it with our finger in the way described; and here, as elsewhere, the sense of touch does not act as a privileged arbiter of the real. I am not suggesting that the sense of touch alone, without the aid of the other senses, cannot enable us to know when a stick is bent and when it is straight: it presumably can, if we take the discrimination that people totally blind from birth exercise; but how it does this is not easy to explain and does not concern us in this paper.

Even if the sense of touch is able to inform us that the stick is not bent, our perceptual problem would yet remain: we shall have to explain why it is that the stick looks bent and why our sense of sight is here providing us with wrong information. Philosophers, because of the phenomenon of refraction and because of what they have considered other visual illusions, have indeed given the sense of touch priority over the other senses: they have thought the sense of touch, unlike that of sight, to be directly in contact with the material world. This is to forget that the sense of touch also has its own "illusions", that there are instances when it too appears to be giving us wrong information about what actually exists. In any case, to say that the sense of touch is in direct contact with the real world, while the sense of sight is not, is to dissociate the object we touch and the object we see. We surely want to say that we touch and see the same object, that it is, for instance, the same table, which our hands touch, that we see before us. We do not want to deny that our sense of sight is in direct contact with the real world, that we *see things* as well as touch them. We are therefore forced to consider the problem of refraction afresh and provide an explanation, which is consistent with the assumptions we ordinarily make, of why the stick looks bent when partly immersed in water.

How do we know that the stick is not actually bent? If the stick is hollow, we can be aware, peering through it, that the stick has retained its straightness while it is partly immersed in water. Again, when we totally immerse the stick we see that it is straight and when we take it out of the water we again see it thus. We know, then, that the water does not actually bend the stick, does not exercise physical force upon it: we know that we have before us a stick which, for want of a better term, we shall say is only *visually bent*. As naïve realists, we cannot say that the stick is actually bent in water, as Professor Ayer suggests naïve realists should do. If we say this, we would have upon ourselves a whole host of difficulties: how are we to explain where the stick bends? We shall have to say that it is only the surface of the water that exercises force upon the stick. How are we to explain how the stick straightens itself where it was bent before and changes the points at which it bends on further immersion into water? To explain this we shall have to say that the water below the surface exercises a force in the opposite direction and unbends the stick. Why, again, does the angle at which the stick bends depend upon the angle at which it is immersed into the water? Why does the stick, when put in exactly at an angle of 90 degrees to the surface of the water, not bend at all? As we move the stick away from its vertical position, it begins to bend, and the angle of its bending increases. But on further increasing the angle away from the vertical, the stick appears to unbend itself, as it were, and, as the part of the stick above the water is level with the surface of the water, the stick is again straight. At this point, the stick is, of course, totally immersed in the water, though almost at a level with the water's surface. We have already noted that when the stick is totally immersed in the water it is completely straight. How are we to explain the erratic "behaviour" of the stick? To say that the stick actually goes through all these changes of shape will get us into insoluble difficulties. We have a meaning for the words "changes of shape" applied to situations when, on the exertion of physical force, a straight bar, for instance, is bent: for the bar to regain its earlier shape or to be altered in a new way, physical force must be applied. To say that the water bends and unbends the stick, depending on the angle at which it is immersed, and sometimes does not exercise any force on it at all, is to attribute strange and unaccountable causal powers to the water. The water, moreover, bends things without exercising a force that we can feel in any way: when we put an arm into the water we do not feel any physical force upon it. Yet it too looks bent like any other physical thing. We cannot then say that the stick really changes its shape when immersed in water. We all know that still water does not exert physical force in this way. Therefore, if we wish to be consistent in the language we use, we cannot employ the phrase "changes of shape" in describing what happens when a stick is immersed in water.

What shall we do? Some philosophers have concluded from this and similar phenomena that immediate perceiving does not, on all occasions, give us the real qualities of things; they have been led to make a distinction between veridical and delusive perceptions. Once we do this, however, it is clear that we cannot remain naïve realists. According to naïve realism, as I interpret it, to say of a thing that it is perceived, i.e. seen or touched or heard or smelt or tasted, implies saying that it actually exists and that it actually possesses the character that it is perceived to possess. No distinction is made in naïve realism between normal or veridical and abnormal or delusive perceptions: all perceptions, according to naïve realism, are veridical. Since we cannot, as Ayer suggests, say that the stick actually bends, what can we say? We can, of course, give up naïve realism and adopt the terminology of sense-data, but this is not what we intend to do.

Considering the large number of philosophers who have dealt with the problem of refraction, it is a remarkable fact that none of them have noted that the stick in water does not in many respects look like an ordinary bent stick. In fact, in its *essential* characteristics the partly immersed stick does not resemble a bent stick. This, strangely, is what philosophers have not mentioned at all. Professor Austin has said in lectures[1] that a stick partly immersed in water does not look exactly like a bent stick out of the water. He wittily pointed to the fact that we could not help seeing the water. All this is true and needed saying: we are not deceived by the phenomena of refraction, for they occur in conditions which can be distinguished by us as conditions for refraction. When water, hot air, the use of magnifying or diminishing lenses cause objects to look to us differently from the way they usually look, we are not left without means of finding out that we are seeing in other than usual conditions, that we are in fact seeing things in water or in hot air or through extraneous lenses. Notwithstanding this, our main problem still remains unsolved. The final question is not whether we are or are not deceived by refraction but whether we are entitled, because of the facts of refraction, to use words like "bent" and "straight", and other words referring to shape, of material things: this is how the facts of refraction become philosophically disturbing.

When we say of a thing that it has a certain shape, that it is straight for instance, we are, according to ordinary assumptions, describing the thing by one of its real characteristics or qualities. When the same thing, without undergoing what we call a real or physical change, is seen under different conditions as bent, the question arises as to whether the terms "bent" and "straight" properly apply to the thing at all. We are not willing

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[1] I refer to the lectures entitled "Sense and Sensibilia", first given at Oxford in 1946 and repeated in Michaelmas Term 1955. Though these lectures are no doubt famous, it is greatly to be regretted that they have not been published. Professor Austin, in my opinion, has shown more decisively than any other philosopher that the sense-data theory of perception has no logical ground whatever: the "illusions" we are faced with in perception do not warrant the sense-data theory; nor does it even remotely answer the perceptual problems actually raised by the facts of illusion, so called.

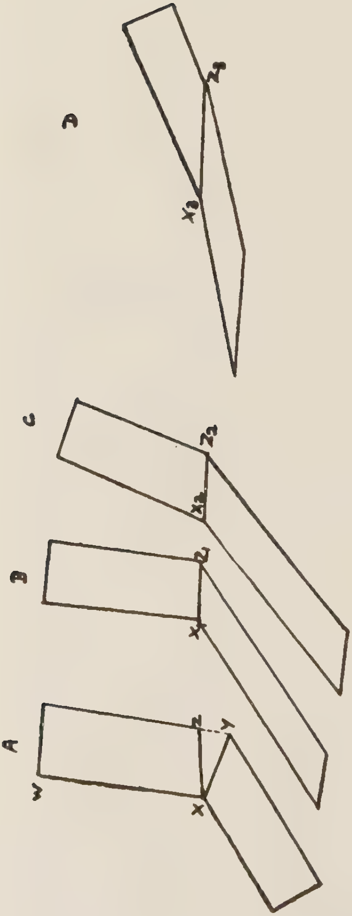
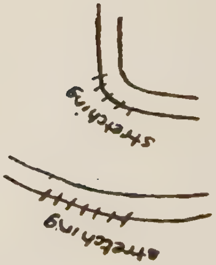


Fig. 1



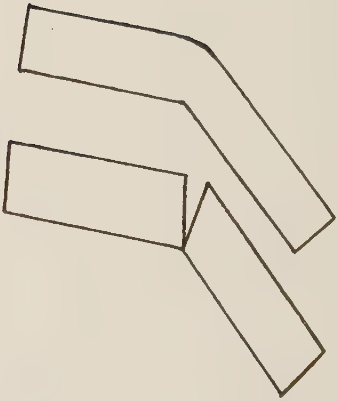


Fig. 3

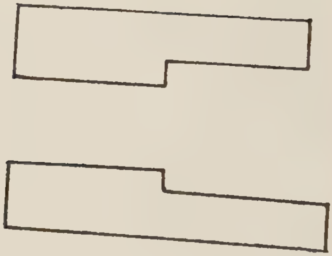


Fig. 4

to say of water or any liquid that it is, for example, spherical or box-shaped, because liquids have no shape themselves but take on the shape of the vessel that contains them. But of solid things, especially those that are rigid, we do want to say that they have definite shapes, that they are, for instance, straight or bent, round or square. The facts of refraction, and indeed of perspectival distortion, create difficulties in the way of our attributing shapes, simply and without qualification, to material things. That we do use words like "bent" and "straight" as simple attributes in ordinary and scientific discourse there is no doubt. The question for philosophy to decide is whether we are justified in so using them.

The stick in water, as we said in a previous paragraph, does not resemble in many respects a bent stick. We add that in their *essential* characteristics refracted sticks and bent sticks are visually different. In Figure 1, A is a drawing of a bent stick and B, C and D are drawings of refracted sticks. It is not easy to bend a stick at two precise points: there is usually a stretching at the outer side of the bend (vide Figure 2). We can avoid, however, the stretching on the outer side if we compress the inner side of the bend: we have thus, with difficulty, made our stick A bend at two precise points to make it resemble as closely as possible a refracted stick. Yet the differences between bending and refraction remain. If the stick A is bent at a point X on one side, this involves an equal bending of the stick at a point Y on the side behind: the angle WXY must form 90 degrees. The stick cannot be bent at the points X and Z, which when joined form with the side of the stick an angle less than 90 degrees: an attempt to do this will crack or stretch the back of the stick (vide Figure 3); in neither case will the stick be said to *bend at the points X and Z*. The refraction of the stick occurs, however, at the points X and Z, points on a level with the surface of the water: the angle formed by this horizontal line and the side of the stick is always less than 90 degrees.

Imagine a stick bending at the points at which the stick D (Figure 1) is refracted! It is impossible for a stick to bend at points X_3 and Z_3. As the stick nears the vertical position on immersion the angle WXZ gets nearer 90 degrees and the refraction that takes place resembles bending more closely. But no instance of refraction does or can take place at points which when joined form right angles with the sides of the stick. We must not forget that a stick is refracted only when it is immersed at an angle with the water's surface. If it is immersed at right angles to the surface of the water, the immersed portion of the stick does not appear to bend. The stick is always refracted therefore at points which when joined form with the side of the stick an angle of less than 90 degrees. A stick cannot ever bend *exactly* at two such points. There is a logic or necessity in bending to which all bending must conform. If we attempted to bend a stick at the points at which refraction takes place, the back of it, as we have illustrated in Figure 3, would crack, not bend, or would stretch. In any case, the stick will not bend neatly at the points at which it is refracted and it is the preciseness with which refraction takes place at these points that distinguishes it from bending.

We can also point to the fact that the refracted part of the stick is always narrower than the part above the water. That the refracted part of the stick is narrower than the part unrefracted shows us that what we see before us is not a bent stick. If it is a bent stick that we see, let us try to imagine how it would look if it were unbent or straightened. Figure 4 is an illustration of how the stick under discussion would look if it were straightened. We could see from this that what we have before us is not a straight stick bent. The important point is that refraction and bending are visually different, though they are also visually alike in some degree and to such a degree that they can be confused.

Refracted sticks, therefore, turn out on closer examination to be essentially different from bent sticks. Refracted sticks, in the last analysis, *do not even look bent.* If sticks were refracted at points where they could bend and there were no visible difference between refraction and bending, as philosophers who hitherto discussed this question have assumed, then our problem could not have been treated in this way.

II

Our problem is only partly solved, it may be rightly said, for a fresh philosophical difficulty arises. Though we may not ask, "Why does a straight stick look bent when put into water?" because, strictly speaking, it does not look bent, cannot we inquire, "Why does a straight stick not look straight when put into water?" Though a refracted stick is not a bent stick and does not look like one in all its details, it also does not look in any case like a straight stick. Why does a stick, then, when partly immersed in water, look otherwise than it actually is?

The stick is actually straight: why does it not continue, therefore, to look straight when half immersed in water? If perceiving always gives us characteristics which are real or actual, why does the stick not continue to look straight even after it is thrust into water? It must be admitted that the stick partly immersed in water does not look as it does out of the water, or as it does when totally immersed. How are we to explain this change of appearance?

The problem is indeed a difficult one and we must be careful at the outset to understand exactly what it is. There are other problems connected with apparent changes in the shapes of things which we have to be careful to distinguish from the apparent change of shape effected by refraction. When we look at a thing from different angles, for example, it appears to us to have different shapes: as we all know, a penny looks elliptical and not round when viewed obliquely. We have to be particularly careful when we deal with the problem of refraction because there appears to be some confusion among philosophers about the very facts of refraction[2]. It has been said that the stick appears bent when viewed from certain angles and that viewed from other angles it appears less bent or even straight. This is contrary to fact: the stick looks bent from whatever angle you look at it: that is, as long as you look at it through the surface of the water and not through any of the sides of the tank in which the water in contained. It is true that from some points of view it is less easy to see that the stick is bent than from others: if you look at the stick from the direction towards which the stick bends, you will hardly see the bend. The same is true, however, even when we look at actually bent things. If we bend a match, for instance, and look at it from the direction towards which it bends, it is not easy to distinguish that it is bent. It is always easiest to see the bend of something if one looks at it from a side. No one intends to say that the match is not really bent, because we do not distinguish its bend from a certain point of view, or that its degree of bending depends upon the point of view from which it is seen. We are willing to concede that why the actually bent match and the apparently bent stick do not appear bent, when we look at them from a certain point of view, is a perceptual problem: but it must be clear that it is a different problem from the one we are discussing here. In refraction we are concerned with why a stick which is actually straight looks bent from any point of view at all: we are not concerned, for the moment, with the different perceptual problem of why its apparent bend is better visible from certain angles than from others. The difficulty about refraction is precisely that the partly immersed stick is exactly like the actually bent match in this respect: its apparent bentness is as much a public fact as the actual bentness of the match and is independent of the point of view of the observer, though, of course, it is not independent of the angle at which the stick is immersed in the water. As everyone knows, the stick does not look bent when immersed vertically in the water.

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[2] Vide Toulmin: *The Philosophy of Science* p. 61: here it is stated that the apparent position of the stick depends upon the angle from which it is viewed.

How are we to explain why the stick looks bent? Why do the two parts of the stick not form a straight line? It must be made clear at the outset that we cannot solve our problem by making a distinction between veridical and delusive perceptions and by characterizing one of them, namely, seeing the stick partly immersed, as delusive. This is what philosophy, faced by the facts of refraction and what it has considered other visual illusions, has hitherto done. Not being able to find a logical justification for the distinction between veridical and delusive perceptions, philosophers have gone on to deny that we are ever in contact with the real world: they have supposed that we sense sense-data or have ideas when we perceive and they have raised for themselves one after another the familiar solipsistic problems. By doing this, philosophy has in fact avoided its real tasks and created for itself fictitious difficulties. By throwing doubt upon the thesis that our senses are in direct contact with the real or material world, it has rejected our only available means of knowledge of this world. The facts of refraction are equally part of our knowledge of this world: it is these facts which show us that media have different optical properties; perceptions of sticks partly immersed in water are as veridical, therefore, as perceptions of sticks wholly out of the water. That things, when they are partly immersed in water, look different from the way they look otherwise does not show us that some of our perceptions are delusive.

What, then, do the facts of refraction prove? Do they show us that the term "straight" cannot be applied in a simple and unqualified way to the stick? The stick is straight when seen under one set of conditions and not straight when seen under another: therefore, straightness or a real shape cannot be attributed to the stick independent of the context in which it is seen. Because the shapes of things look different when the things are seen in different conditions, are we to go on to maintain, as Professor Austin did in his lectures, that things cannot be said, in a simple and unqualified sense, to have real shapes?

Though Professor Austin did wish to say of the stick in water that it was straight, his criticism of the term "real shape", as it applied to things seen through microscopes and telescopes, implies that shape-words in general could not be predicated simply of things. "Straight", "bent", "round" and "square" are more basic terms, it is true, than "horse-shaped" and "cat-shaped", which are the terms Austin dealt with, but they are equally terms of shape. If "horse-shaped" and "cat-shaped" cannot be predicated in a simple and unqualified way of things, then the more basic shape-words also cannot be used in this way.

This is then one possible direction in which we may proceed in order to try to solve our problem of refraction: we can say, not that the perception of the stick in water is delusive, but that the word "straight" cannot be applied to the stick when seen in this condition. To the question, "What can be said of the shape of the stick in this condition?", we can answer that it can be said to be "bent" or be "bent-like". Our problem of refraction, according to this argument, is caused by the fact that we use terms like "straight" and "bent" of the stick, as though these terms applied to the stick irrespective of the conditions under which it is seen. Once we give up speaking as though shapes were intrinsic qualities of things we shall solve or rather dissolve our problem of refraction.

It appears to me that this answer has many serious disadvantages. Besides rejecting as invalid the way shape-words are used in ordinary language, it takes no account of the fact that the stick partly immersed in water is not bent and can be shown not to be bent. If the stick in water really looked like a bent stick, then there would be greater need to be dubious about the simple application of our shape-words. Since the stick in water, on closer examination, does not look bent, it does not seem necessary to go as far as rejecting ordinary usage. As was pointed out before, we do not use words like "straight", "bent", "round" or "square" of liquids, because liquids change their shapes depending on whether they are seen in straight, bent, round or square containers. We similarly do not use shape-words of air or of other gases. But solid things like sticks, rocks, trees, have definite shapes. The shapes of these things do indeed change: sticks break or get bent, rocks change their shapes when they decompose and trees when they grow. These changes are explained, however, by physical or natural processes:

what these physical or natural processes effect are changes of shape; the sticks, rocks and trees change from one definite shape into another.

When a stick is seen partly immersed in water it does not, as we have amply pointed out, change its shape. What is it then that changes? How are we to describe what has happened? If we simply say that the word "straight" cannot be used of the stick, we are not making any use of the fact that we are now seeing the stick *in different conditions*: that we are now seeing the stick partly immersed in water where before we saw it out of the water. We can see the water, as was pointed out before, and see that the stick is partly immersed in it. If in order to solve our problem we simply say, as recommended, that the stick in itself is not straight, we are ignoring any significance our seeing in altered conditions may have. If we sometimes saw a stick straight and at other times saw it bent without any change occurring in our conditions of observation, then, it is agreed, we would be reduced to saying that shape-words cannot be predicated of the stick. It may be argued, however, that what Professor Austin is maintaining is not that shape-words cannot be attributed to things but that they cannot be attributed without qualification: according to him, we can predicate "straight" of the stick if we add the qualification that the whole of it is seen in air: we cannot predicate "straight" without such a qualification.

The question we wish, however, to raise is, "What sort of qualification is this?". What do we mean when we say that the stick is straight *if it is seen* out of water, in air? We can say, of a rubber ball, that it is round if it is not being trod upon; or, of a tree's branch, that it is straight when the wind does not press on it. When we qualify our predication here, we do not imply that the ball is not round *in itself* or that the branch is not *really* straight. In the case of the partly immersed stick, on the other hand, when we say of it, "It is straight if it is seen out of the water", we are recommended to imply that it is not straight in itself, not really straight. Our qualification in the case of the ball and the branch does not, again, preclude us from saying that the ball is really flattened when stood upon and that the branch is really bent by the wind. In the case of the stick, however, when we say it is seen bent when partly in water, we cannot conclude, as we did in the case of the branch, that it is really bent. The kind of qualification made in the case of the stick is thus of a radically different kind.

To solve our problem by qualifying the words "straight" and "bent" in the way suggested leads us into difficulties: the qualification made does not conform to the usual kind of predicate-qualification that we find in the language. What is more, the qualification contradicts the assumption that what is perceived is independent of the perceiver and the conditions under which the perception takes place: this assumption is contained in the naïve realist view of perception and, as I hope to show, it colours ordinary-language statements about perceiving and its objects.

We feel the need for an explanation when a thing looks otherwise than it usually does. Suppose someone were to say of a thing, "Its shape is now seen to be different", do we not always inquire, "Why is its shape seen to be different? What explains this?" Why do we expect an explanation? Our need for an explanation is satisfied if we are told that the thing itself has changed or has been altered. Why are we satisfied by this explanation? We are satisfied by this explanation because it gives us a reason why the thing looks different: it looks different, says the explanation, because it has changed, because now it *is* different. The explanation satisfies because it preserves the assumption that a thing looks as it is, that when we perceive a thing we are aware of its real characteristics.

This assumption is fundamental to naïve realism: according to this perceptual theory, whenever we see, touch, or otherwise perceive, we are aware of a material thing and its characteristics. Now, by definition, a material thing is independent of the perceiver, for the same material thing is perceived by more than one perceiver: it is an object which furthermore exists even while it is not being perceived. This material thing does indeed change when other material things or forces affect it, but our perception of it does not in any way alter it. That is why, if we see it differently, we do not say that it has changed but that it *appears* to have changed. The word "appears" and its equivalents, "looks" and "seems", when used in this context, signify that the object has not really changed: they imply in fact that the object is the same or retains the same characteristics even though it is seen as being different. To say, therefore, that the characteristics of objects, their shape, for instance, cannot be defined apart from the context in which these characteristics are perceived, is to ignore the purpose for which the words "looks", "seems" and "appears" are employed in these contexts. Furthermore, if the shape of a thing is not independent of the conditions under which it is perceived, can we consider the thing to be so independent? If we consider that the thing is independent of the conditions of perception while its shape is not, then we have fatally separated the thing from its characteristics, a separation which we shall find hard to justify. If the thing itself is equally dependent, like its shape, upon the conditions of perception, then the object we perceive is not a material thing by definition and we are not really naïve realists: we are nothing but sense-data philosophers in disguise.

We cannot give up the thesis that the characteristics or qualities of a thing remain unaffected by the conditions of perception without adopting sense-data positions or getting involved in Lockean-like distinctions between things and their characteristics. However hard it is to solve the problems raised by the so-called illusions, we must nevertheless try to deal with them without abandoning the naïve realist point of view: to retreat from its firm and clear doctrines is to throw oneself back into the scepticism from which modern philosophy is trying hard to escape.

The stick in water, then, is not bent: neither the stick nor its shape has changed. We have too many advantages to forgo if we abandon this view. The stick out of the water is seen straight: totally immersed in water it is again seen straight; in other media or through lenses, if the whole of it is seen in one medium, the stick is again seen straight. It is only when the stick is partly seen in one medium and partly in another that it is seen bent. Even in these conditions it is not always seen bent: if, from one medium, it is vertically immersed into another, it is still seen straight. It is only when the stick is immersed at an angle with the surface of the water that it appears bent: this is the only trifling condition in which it appears other than it really is. We have surely too much to our advantage to need to abandon naïve realism or adopt the defeatist position that the shapes of things cannot be defined apart from the context in which they are seen. We have already pointed out that we can also perceive the conditions under which we are seeing: we can see that part of the stick is in water and that part of it is above the water and even that the part above the water is at an oblique angle with the surface of the water; we can thus see every detail of the disturbing conditions which make the stick appear otherwise than it actually is.

All this, it may be said, only points to the fact that refractions are in no sense to be called illusions: they are as veridical as perceptions of sticks in normal conditions. We have not yet solved the problem, however, of whether, because of the phenomena of refraction, we can call the stick in itself straight or bent: this is our major difficulty. The answer that suggests itself on first thoughts is that we should re-define straightness in such a way as to apply to the refracted stick. We have amply shown, after all, that the stick, strictly speaking, it not bent, because bending never takes place at the points at which refraction occurs. Why can we not therefore define straightness in such a way as to apply both to the stick as it is seen out of the water and to the stick as it is seen partly in the water, define it in terms of the common characteristics which straight and refracted sticks share? We could then say simply that the partly immersed stick looks straight, according to our new definition of straightness.

This does not seem to be a satisfactory way of solving our problem. If our refracted stick, on closer examination, does not exactly resemble a bent stick, there is no difference between it, as it looks, and a *crooked* stick of exactly that shape. If our definition of straightness covered the kinds of shape that refracted sticks take on, we should have to include many actually crooked sticks in the same category. To define straightness to include crooked sticks of certain kinds would be to travesty the notion of straight: it would furthermore make the term "straight" rather technical and complicated and hardly suitable for use in ordinary language.

We cannot then re-define straightness and hope to solve our present perceptual problem. The position, or the place in which a thing is, is a relative characteristic, something that is defined in terms of *other* things; but the shape of a thing is an absolute characteristic, something that is defined in terms of the thing alone. Yet the notions of position and shape are related: the shape of a thing is determined by the spatial relationships of the parts of that thing to each other. Though the relationships concerned are internal ones and do not involve other things, yet the relationships are spatial. It would be curious if apparent changes of spatial position caused more than apparent changes of shape. We must be careful then not to admit that a real change of shape has occurred. But how shall we avoid admitting this? We seem to have tried all possible ways out and none of them has succeeded.

Can we not solve our problem by making a distinction between the seeing of a thing and the seeing of its shape? The stick itself is seen: actual parts of the stick are seen above water and other actual parts of it are seen in the water; we can say without a doubt that we are seeing the stick because we are seeing its parts. To see the shape of something, however, it is not sufficient to see its parts: we must see the parts of the thing *together*. There are many occasions on which we can be said to have seen a thing but not to have seen its shape. Suppose something were shown to us in parts, each part uncovered at a time but never two parts together, until all the parts of the object are seen. We can then truly say that we have seen the whole object: yet it is also true that we have not seen its shape. We can again be said to see the whole exterior of a huge building if we walk round it and yet not be said to see its shape. To see the shape of something all parts of its surface must be seen in a unitary from or *Gestalt*.

On some occasions, then, like the ones we have discussed above, we can be said to see a thing and not see its shape. We must not forget, however, that when we see the parts of a thing which are uncovered to us one at a time, we do see *the shapes* of *these parts,* and that, while we walk round a building, we do see the shapes of its different portions. If we do not see the shape of the whole thing, it is because under these conditions we do not see the thing as a whole: we do not see all the parts of the thing together or at the same time. The parts of our partly immersed stick, however, are seen at the same time. We seem thus to be left with our problem yet unsolved.

III

When we are faced with a perceptual problem (i.e., when we find that a certain characteristic of a thing appears different in different conditions of perception), there are two main ways in which we can attempt to solve it. We can say either that the characteristic of the thing in question is a relative characteristic or that our perception of it under certain conditions is faulty: we can qualify the characteristic or, keeping that constant, qualify the perceiving. With regard to the problem of the shape of things, we have tried up to now to qualify this characteristic: we have tried to say that the term "shape" is relative and is to be defined in terms of the conditions under which it is perceived. This solution has not been satisfactory because it has run counter to our fundamental assumptions. Let us now try the other of the possible ways of solving perceptual problems: let us try to find some means of saying that in certain conditions of perception our perceiving of shape is in some way faulty.

Before we attempt to solve our problem in this way, let us ask ourselves again what exactly our difficulty is. The question that puzzles us is why the stick looks bent when partly immersed in water at an angle. Certainly, the part of the stick in water does not look bent and the part above the water also retains its straightness: both parts, taken separately, are straight; but taken together they are not. We cannot say that in these conditions the whole stick looks straight: the whole stick simply does not look straight. The question we may raise, however, is whether, because they are seen in different media, both parts *should* be taken together. We must question whether our seeing a thing partly in one medium and partly in another is a proper way *of seeing its shape.* Since we cannot say that the stick looks straight, that it looks its real shape under these conditions, we must solve our perceptual problem by throwing doubt upon the validity of our seeing the stick's shape in these conditions. We can say that we cannot in these conditions see the shape of *the whole stick,* though we can see the shapes of its two parts, the part above the water and the part below it.

We must clearly point out, in case it be misunderstood, that we have not characterized our perception under these conditions as delusive: we do see what is actually there; we do see actual parts of the actual stick. And we do see the *real shapes* of these parts, and the parts are really straight and are seen to be straight. But in these conditions we do not see the shape of the stick as a whole. Our perception of the whole stick is faulty because both parts of the stick are seen separately: though both parts are seen *at the same time,* they are not seen *together,* in a way we shall soon specify. We do not see the shapes of the whole stick, therefore, because we do not see the stick as a whole: we see it in parts; each part is seen separately or through a separate act or kind of seeing. Our seeings occur at the same time but are not on that account to be taken as one seeing.

In an earlier paragraph, we cited occasions when we could be said to see a thing but not to see its shape. We see the whole of a building when we walk round it, but we do not see its shape as a whole because our seeings of the parts do not occur together or at the same time. The shape of a three-dimensional object cannot, strictly speaking, be said to be seen by us because we cannot see all parts of the object at the same time: we cannot include within one act of seeing the whole object. On some occasions, however, all or most of the different parts of a thing can be seen by us at the same time: this happens if we look at a three-dimensional object with a mirror behind it; we can now see most or perhaps all parts of the object *at the same time.* Yet we would not say we see the object *as a whole,* as the one object it actually is, because the part of it seen in the mirror is visually disconnected from the rest. Though the seeings of the parts occur at the same time, we do not on that account consider these seeings as one seeing: they are deservedly to be called separate seeings or kinds of seeing.

We can, even at one moment, have two seeings. In a previous article[3] we said that, when we see double, we see one thing twice. Here again, at one moment in time we have two seeings. Though talk about seeings or acts of seeing is not common in our language, we are forced to introduce it in order to avoid saying, in cases of double vision, that we are seeing *two things.* We are not seeing two things when we see double, but are seeing one thing twice. From saying this the transition is easy to the talk about seeing in the plural, to acts of seeing.

Ordinary language has developed in order to deal with the situations in life we find usually and commonly. Its equipment is not sufficient to deal in a precise way with extraordinary situations. Science is necessary precisely because of this. Science does not, or at any rate should not, invalidate the distinctions made by ordinary language: it should sharpen these distinctions and make new ones which are consistent with the old. Cases of refraction, reflection and double vision are comparatively unusual and we are content in our ordinary language not to make special allowance for them. To describe our seeing of ourselves, for instance, in mirrors, we indifferently use in ordinary language one of the following expressions: we say, "I see my reflection" or "I see myself by means of a mirror, through a mirror" or simply "I see myself in a mirror". These expressions are by no means equivalent to one another, though they are used indifferently of the same situation; nor are some of the words in them used as they ought to be used. When I say, "I see myself *in* the mirror", I do not use the word "in" in the same way as I do when I say, "I see the chair *in* the room". Again, "To see my reflection" and "to see myself" are not equivalent in meaning if we consider, not the use to which the phrases are put, but their logical form alone: the meaning of the word "my" in "my reflection" is not the same as when, for instance, we use it in "my body" or "my arm".

To deal with these unusual cases of vision we need, then, to sharpen the distinctions we already have in ordinary language and to make new ones. The science we have been used to since Newton has employed the term "mirror image" of what we see in mirrors and has distinguished between *things* and that which we see in mirrors. It tells us that what we see in water is not the real stick, for instance, but an image of it. These distinctions, as we have shown, run counter to the logic of ordinary language. In ordinary language what we see, when we do, is a thing which is in the world and is where it is seen to be. The image in the water and in the mirror are not supposed by the scientist to be things and are, therefore, not where they are seen to be: in fact, they are "in the mind" and not in the world. We may well question, therefore, whether we *see* them, whether the word "see" is used here in the way we use it in ordinary language. We need a science of seeing which does not contradict the assumptions and distinctions which we usually make: it must deal with the extraordinary cases of vision by sharpening and refining the terms of ordinary language and not by distorting them.

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[3] "Naive Realism and Illusions of Reflection", this *Journal,* Dec. 1957, pp. 155-169.

We must not forget that when we see the stick in water we see *through* the water: because part of the stick is actually in the water we have to see through the water if our vision is to reach it. Similarly, though we do not bother to remind ourselves of it, we see the part of the stick above the water through the air. In fact, the further away we are from the stick, the more air we see through when we see it. Because air is usually completely transparent we tend to neglect its presence, but we must not forget that it exists and is as material as other things. Seeing through air, seeing through water, likewise seeing through other media or through lenses, are different kinds of seeing. In ordinary language we have not learned to qualify seeing. We say we see things *in water,* without taking special notice of the fact that, because the thing is in water, we see *through water.* We qualify the thing, its position, when we say that it is in water, but we do not qualify our way of seeing it, by adding that we see through water.

In ordinary language we neglect to mention the different kinds of seeing for the further reason that the things themselves do not always look altered when we see them differently. The shape of the stick totally immersed in water does not look different from its shape in air. A tree preserves the same shape in general when looked at from a distance or from close quarters: things seen by the naked eye or through telescopes are roughly similar in shape. In ordinary language we are content if we can approximately describe reality.

To make our ordinary language equipment more precise and capable of dealing with the problem of illusion, we must learn to qualify seeing. When we see a thing in water we see it differently: the thing is not now different but we now *differently see.* The word "differently" adverbially qualifies the seeing and not the thing seen. We had first tried, in dealing with the illusions of perception, to qualify the thing: and, finding that we could not (because things do not change unless they are physically affected), we went against our common-sense assumptions and posited images or sense-data as the objects we see, in an attempt to solve our problem. We did not, for some reason, have the courage to qualify our seeing and distinguish it into different kinds.

When we see through water, through air, through different media or through different lenses, we see differently, see in different manners or ways, have different kinds of seeing. These different kinds of seeing may occur together or at the same time, but do not because of this become the same seeing or the same kind of seeing: when we see a stick partly immersed in water, i.e. when we see part of it through air and part of it through water, we are differently seeing each part of it. We do not apprehend its shape when we see it thus because we do not unitarily see the whole of the stick: each part is differently seen, apprehended through a different kind of seeing. Seeing by means of a mirror is likewise a different kind of seeing from seeing a thing directly. When we come to cases of double vision, we are faced not with different kinds of seeing occurring together but simply with different seeings occurring together: two seeings of one kind occur at the same time.

Talk about seeings, about different kinds of seeing, no doubt sounds unfamiliar: in fact these ways of speaking are unfamiliar, these distinctions not being found, as we have noted, in ordinary speech. Some distinctions such as these, however, must be made in philosophy if we are to solve the problems raised by the so-called illusions of perception[4]. On the basis of these distinctions we can explain why the stick partly immersed in water does not look its real shape. The stick does not look its real shape when seen partly in air and partly in water, not because it has changed or its shape has changed: nor yet because in the water we do not see the real portion of the stick but an image a few inches above it. The stick does not look its real shape because we differently see the two parts of the stick: our seeing of one part is of a different kind from our seeing of the other.

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[4] The case for distinguishing seeing into different kinds is, I believe, given support in Professor Austin's account of perception. He states, in his lectures referred to previously, that the phrase "indirectly seeing" as opposed to "directly seeing" has application: he refers to seeing in mirrors as an instance of "indirectly seeing".

We do not commit ourselves in this account to saying that the shapes of things are dependent upon the conditions of perception, that they cannot be defined independently of the medium in which they are seen. Our stick is straight and its straightness is independent of the medium in which it is seen. In air it is seen straight and in water it is again seen straight. When placed in other media too it does not appear other than straight. We can rightly say, therefore, that the term "straight" can be applied to the stick independent of media, independent of the conditions of perception. If it is not seen straight when part of it is seen in air and part in water, this is not because the term "straight" does not apply to things independent of media: it is because, in these conditions of perceiving, we do not see the two parts of the stick *together,* in the sense not of seeing them at the same time but of seeing them by means of one and the same seeing.

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THE ONTOLOGICAL PROOF REVISITED

By Nicholas Rescher

> **"I began to ask myself whether**
> **there might be found a single**
> **argument which would require no**
> **other for its proof than itself**
> **alone, and would suffice to**
> **demonstrate that God truly exists."**
> **—St. Anselm**

I. Preface

For years now it has been the almost universal practice of philosophers to dismiss Anselm's ontological proof of the existence of God. Perhaps the most recent significant exception, surprisingly enough, is the brief acceptance of this argument by the young Bertrand Russell. In his essay on "My Mental Development", Russell records:

> "I remember the precise moment, one day in 1894, as I was walking along Trinity Lane [in Cambridge], when I saw in a flash (or thought I saw) that the ontological argument is valid. I had gone out to buy a tin of tobacco; on my way back, I suddenly threw it up in the air, and exclaimed as I caught it: 'Great Scott, the ontological argument is sound'."[1]

This quaint episode apparently constitutes a high-water mark for the ontological proof in recent philosophical history. It is now quite thoroughly and completely out of favour, among philosophers of religious and anti-religious persuasions alike.

The major apparent reason for this general rejection of the ontological proof is a number of seemingly decisive and insuperable objections which have been directed against its logical validity in the first instance, and its theological serviceability (even were it to be valid) in the second. These objections have now beeen advanced so repeatedly and so persuasively as to have become classical. Their aggregate effect has proved quite fatal to the ontological proof. The purpose of this paper is to show that it is possible to reformulate the ontological argument in such a way that all of these traditional objections to it lose their force, so as to render the argument once again worthy of serious philosophical consideration.

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[1] *The Philosophy of Bertrand Russell,* edited by P. A. Schilpp (Evanston and Chicago, 1944), pp. 3-20, quoted from p. 10. "I shall venture to add an observation, that the argument a priori [for God's existence] has seldom been found very convincing except to people . . . who have accustomed themselves to abstract reasoning, and who finding from mathematics, that the understanding frequently leads to truth, through obscurity, and contrary to first appearances, have transferred the same habit of thinking to subjects where it ought not to have place." Hume, *Dialogues Concerning Natural Religion,* part IX.

II. The Ontological Proof and Some Objections

It will be convenient for my purpose to consider the ontological proof in the format given to it in Descartes' Fifth Meditation:

> "I clearly see that existence can no more be separated from the essence of God than having its three angles equal to two right angles can be separated from the essence of a triangle. And so there is no less absurdity to our conceiving a God (that is, a Being supremely perfect) to whom existence is lacking (that is, to whom a certain perfection is lacking) than to conceive a mountain that has no valley."[2]

In this Cartesian version, the ontological proof has two steps: (1) it is posited that the nature of the concept of God (i.e., the definition of the term) is such as to embrace existence within its scope, and (2) it is contended that, as a consequence, to deny the existence of God is to make a statement that is logically self-contradictory. Thus on Descartes' formulation, the ontological proof concludes God's existence *deductively* from the definition of the term.

From the very first, serious objections have been advanced against this argument. Three criticisms in particular attained to the status of classical objections, and these seem to continue to carry the main burden of justifying the widespread current rejection of the ontological argument.

The first objection turns on the logic of the definition of the term "God" that is basic to the ontological proof. The argument rests upon an explicit definition of this term *per genus et differentiam* which is such as to include existence within the set of attributes that constitute the defining, essential attributes. The logical admissibility of such a procedure depends upon the thesis that *existence* has the status of a predicate, i.e., an attribute-denoting term. But, as logicians from the time of Hume and Kant have consistently and convincingly argued, existence must not be classified as an attribute.[3] This being so, neither can it be an essential attribute, and so cannot qualify as an item to be included in the defining content (i.e., intension) of a term.

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[2] *Meditations on First Philosophy*, Fifth Meditation, translation adapted from that of R. M. Eaton in *Descartes: Selections* (N. Y., 1927).

The second objection protests against the logic of the procedure of "defining into existence". The point and objective that is at issue in a purported ontological proof is, after all, God's existence. To derive this desideratum from the definition of the term is, in effect, to settle the matter by *fiat*. For to insist upon defining a term in such a way as to preëmpt a question regarding it that is at issue, and so to claim the desired consequence as a deductive inference from the definition, is to engage in what is of necessity a circular procedure. The nature of valid deductive reasoning is such that we cannot by means of it extend the limits of our facts, however useful it may be in extending our explicit articulation of the information concerning them: the information expressed in the conclusion must always be also asserted, however covertly, in the premises. If, therefore, an existential conclusion is drawn deductively from premises, these must be such as themselves to presume or presuppose the existential fact in question, so that we cannot actually advance to the establishment of any existential fact other than one already covertly assumed or decreed.

The foregoing two objections to the ontological proof have been derived from logical grounds. The third traditional objection derives from theological considerations. The conception of God upon which the ontological argument proceeds is by no means satisfactory from a religious standpoint. The "God" of which it speaks is a logical amalgam adjusted to the logical convenience of an existence proof, and this makeshift is not recognizably to be identified with the deity worshipped in any operative monotheistic religion. The ontological argument finds God "at the end of a syllogism" and not "in the hearts and souls of men".[4] Thus, even if this proof were logically sound, its theological serviceability would be insignificant. For the "Being" of which it speaks is not properly an object of worship, but a logically expedient makeshift that serves the purposes of *ad hoc* deductive convenience for the sake of an existence proof. *Non in dialectica,* wrote St. Ambrose, *complacuit deo salvum facere populum suum.*

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[3] A useful synthesis of recent discussions is contained in W. Kneale's paper "Is Existence a Predicate?", *Aristotelian Society Supplementary Volume 15* (1936) reprinted in *Readings in Philosophical Analysis* ed. by H. Feigl and W. Sellars (N. Y., 1949), pp. 29-43.

[4] See Chapter 8 of W. T. Stace's fine book *Time and Eternity* (Princeton, 1952). H. H. Farmer writes: "We might put it paradoxically by saying that if anyone succeeded in proving the existence of God he would by that very fact show that he had failed. For a God Who could be thus proved would not be the God under discussion, but something else. The word would in fact have changed its meaning in the course of the argument" (*Towards Belief in God* [N. Y., 1947], p. 30). Similarly J. Baillie holds that "It is not as a result of an inference of any kind, whether explicit or implicit, whether laboriously excogitated or swiftly intuited, that knowledge of God's reality comes to us. It comes rather through our direct personal encounter with Him . . . " (*Our Knowledge of God* [N. Y., 1939], p. 143).

These three lines of thought appear to represent the main grounds for the current, virtually universal disrepute of the ontological proof. I propose here to show that, with appropriate alteration, it is possible to reformulate the ontological argument in such a way that these traditional objections (among others) are obviated altogether.

III. A Revision of the Ontological Argument

The leading idea on which the revision of the ontological proof which I wish here to present is based is the elimination of the argument's use of and dependence upon an explicit definition of the term "God" (e.g. as "the perfect being"). For one thing, the scholastic ideal of explicit definition of all terms *per genus et differentiam* has in modern logic long given way to a more diversified theory of definitions. The theoretical feasibility and pragmatic desirability of alternative procedures of definition is no longer questioned by logicians. And as to the theologians, there is a powerful and long-standing tradition of religious thought that denies altogether the possibility of presenting a satisfactory characterization of God in human language or in human terms (cf. for instance the Scholastic doctrine of analogical predication). There is thus also a strong presumption that a rejection of explicit definition is both warranted and plausible.

But if we give up the employment of an explicit definition of the term "God", how is the ontological proof, which appears to rest fundamentally and completely upon such a definition, to be maintained? To see how a reasonable answer to this pivotal question is to be had, it is necessary briefly to digress into the logical theory of definition, and to see also how it comes about that propositions can be true "by virtue of" definitions.

As has been adumbrated above, the theory of definition of current logic no longer has the simplicity and uniformity obtained by insistence that all terms be accorded an explicit definition *per genus et differentiam.* It has been recognized that many terms cannot be defined in this manner. Certain words, it is generally agreed, cannot be defined explicitly at all, but are such that their meaning can only be grasped on the basis, and within the context, of experience. Specifically this category includes, *inter alia,* those words which themselves designate experiences or aspects of experience: perception words (names of colours, odours, etc.), sensation words (aches, pains, etc.), feeling words (alacrity, lethargy, etc.), emotion words (anger, gaiety, etc.). Such words, which characterize the modes and character of immediate experience, cannot be provided with an explicit definition. The meaning of such words can be learned or indicated only experientially, by referring them to the experiences themselves. There is thus a category of terms which cannot be defined explicitly at all, but whose meaning can be conveyed only experientially.

These experience-presupposing terms cannot be understood by those who lack the experience that is relevant to them, just as a man blind from birth can have no concept of the colour red. This of course is not to say that a person who lacks the experience appropriate to such a term can have no information at all regarding the item for which it stands. A blind man can know that the word "red" applies to a kind of experience that he does not have, and that this experience takes place when certain physical and physiological transactions occur, etc., etc. In short, he can have all sorts of information *about* seeing red. But he cannot see red, does not know what it is like to see red, and so lacks the indispensable requisite for an adequate understanding of the term.

One other point regarding these experience-presupposing terms is necessary. The experiences in question are, to be sure, themselves a wholly subjective matter whose contents cannot be communicated in full. Yet there are perfectly objective *tests* as to whether or not an experience of the type in question has taken place. For example, a person who would not assent to "Red appears different from blue", or who would not demur from "Orange is less similar to red than to green", would thereby furnish us with evidence of gaps or lacks in his colour experience. While we cannot examine other persons' experiences, we can obtain substantial information regarding them by noting how the relevant experience-presupposing terms are applied and employed by them.

On this account, the status of a proposition like "Red is more similar to orange than to green" is in effect a synthetic *a priori* truth. It is synthetic in that it presupposes experience, namely that fundamental experience, indispensable for having a proper grasp of the very *meaning* of the key terms involved.

Thus, since it is presupposed for the *meaningfulness* of the proposition, experience is, *a fortiori*, involved in its truth. On the other hand, the proposition is *a priori* because its truth "follows" from its meaning in the sense that, once a body of experience adequate as a basis for a grasp of the meaning of the terms is given, this is *ipso facto* adequate as a basis for the truth of the propositions in question. It would be incorrect to construe a dissent from such a proposition as expressive of disagreement on the facts, for it is properly to be taken as *prima facie* evidence that its meaning was not correctly understood, i.e. that there exist gaps or deficiencies in the visual experience of the dissenter that preclude a correct understanding of the statements in question.

The reader has doubtless foreseen what is to come. I wish to submit that it is quite in order to apply all of these considerations regarding experience-presupposing definitions to the term "God". I propose to classify the word "God" within the category of words which cannot be given an explicit definition, but whose meaning can only be grasped within experience. On this view, a person whose history is devoid of religious experience simply cannot have an adequate grasp of the meaning of the word "God". And I submit that a person who *does* have a basis of experience adequate to an understanding of this term, also has, *ipso facto,* the experiential basis of evidence adequate to underwrite rational, warranted assent to the proposition that God exists.

Thus God's existence does, after a fashion, "follow" from the definition of the term, not in any deductive sense, but in the sense that that body of experience indispensable to an adequate understanding of this (experience-presupposing) word is necessarily also adequate to validate reasoned assent to this proposition. Thus the proposition "God exists" is, on the present concept, "inescapable in the sense that it is self-evident, that its truth or validity could [I say *must* — NR] be grasped by someone who thought clearly and thoroughly understood the terms involved, without the necessity of deducing it from other propositions".[5] In consequence, a dissent from this statement is *prima facie* evidence of failure to grasp the meaning of the term, and indicates a lack in the religious experience of the person in question. From this standpoint we mark the propriety and correctness of J. J. C. Smart's perceptive observation: "I suggest, the question 'Does God exist?' has no clear meaning for the unconverted. But for the converted, the question no longer arises. The word 'God' gets its meaning from the part it plays in religious speech and literature [I would add *experience* — NR], and in religious speech and literature the question of existence does not arise."[6]

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[5] G. E. Hughes in *New Essays in Philosophical Theology*, ed. by A. Flew and A. MacIntyre (London, 1955), p. 63.

Let us contrast this revision of the ontological argument with the classical version of Descartes. No longer is the purported attribute of existence held to be included among the essential, defining attributes comprising an explicit definition of the term "God". And therefore the statement that God exists is no longer held to be analytic, nor is its denial logically self-contradictory. Rather, the line of reasoning now is: (*a*) The word "God" is not susceptible of an explicit definition but is a term whose meaning can only be had on the basis of religious experience. (*b*) A body of experience adequate as a basis for an understanding of this term must also be adequate as an evidential basis for assent to the proposition that God exists.[7] Therefore, (*c*) a denial of God's existence is indicative of a failure to grasp the meaning of the word.[8] On the basis of this revision, then, the statement "God exists" is no longer analytic, but is synthetic *a priori*. It is synthetic *a priori* in just the same sense as such statements as, e.g., "Orange is more similar to red than green". The meaning of such a statement presupposes experience, and thus its truth does so *a fortiori*, so that the statement is synthetic. But the evidential basis for its truth does not call for any experience *over and above* that demanded by its meaning. Therefore the truth of the assertion can be said to "follow" from its meaning, assuring the *a priori* status of the statement.

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[6] P. 41 of his essay "The Existence of God" in *New Essays in Philosophical Theology*, ed. by A. Flew and A. MacIntyre (London, 1955), pp. 28-46.

[7] In his essay "A Religious Way of Knowing" (*New Essays in Philosophical Theology*, ed. by A. Flew and A. MacIntyre [London, 1955], pp. 76-95), C. B. Martin argues that religious experience can never provide a basis for asserting God's existence on the grounds that "the addition of the existential claim 'God exists' to the psychological claim of having certain experiences must be shown to be warrantable" and that this cannot be done. This argument misfires because experiences themselves can suffice as warrant for the existential theses based upon them, and, as Berkeley so elegantly argued, an extra-experiential link is never forthcoming as a bridge of guarantee between experience and its objects, even in sense-perception.

[8] St. Anselm's original presentation of the ontological proof makes use of essentially the same conception. Anselm writes: "But how has the fool, [who says in his heart 'There is no God' (Psalms LIII 1)] said in his heart what he could not conceive, . . . since it is the same to say in the heart, and to conceive. But . . . there is more than one way in which a thing is said in the heart or conceived. For, in one sense, an object is conceived, when the word signifying it is conceived; and in another, when the very entity which the object is, is understood. In the former sense, then, God can be conceived not to exist; but in the latter, not at all. . . . So, then, no one who understands can conceive that God does not exist; although he says these words in his heart, either without any, or with some foreign, signification." (Quoted from A. Freemantle, *The Age of Belief*, N. Y., 1954).

The line of thought here put forward is a rejection of the practice of philosophers who, behind Hume's standard, hold that a belief in God rests on *faith,* where "faith", they insist, is to be construed as a belief that is not justified or supported by evidence. In *this* sense, "faith" does not enter into the matter at all. For an intelligent and comprehending assent to the proposition that God exists can, on the present account, be forthcoming only where there is a foundation in religious experience to render meaningful the proposition in question, and such experience, if adequate to this task, must *also* provide a suitable body of supporting evidence for the statement.

IV. The Objections Reconsidered

With this proposed revision, the ontological proof is at once freed from the three traditional objections we have considered above. First, the proof is no longer founded upon the supposed status of "existence" as a predicate, i.e. an attribute-denoting term. In giving up the use of an explicit definition of "God", we also abandon the procedure of including existence among the defining attributes which comprise the intension of this term. We have shifted the argument on to a very different logical basis. For it is held, not that existence is an essential, defining attribute of God, but that the experiential foundation indispensable to an understanding of the term "God" must *ipso facto* provide adequate grounds for assent to the proposition that God exists.

The second objection, that the deductive character of the ontological proof necessitates an assumption in the premisses of the very point at issue in the conclusion, is also averted in the revision. The modified proof here presented no longer depends on the deductive consequences of an explicit definition, but on the fact that the experiential basis indispensable for an understanding of the term must inevitably comprehend the evidential grounds for assent to certain propositions in which the term occurs. In the face of a dissent from "God exists", as in the face of dissent from "Yellow is lighter than purple", we are forced to a like interpretation, *viz.* that the key terms in question are not properly understood. And, of course, to say this is not by any means the same as the circular procedure of claiming that the truth of these statements follows deductively from the formal definitions of the terms in question. Indeed, on the present view these terms have no explicit definitions, and can only be "defined" experientially.

Finally, the third traditional objection, *viz.* that the ontological proof hinges upon interpreting the word "God" in a religiously trivial and irrelevant sense, also falls to the ground. For the revised proof depends pivotally upon the thesis that the term is not defined in any logically *ad hoc* manner whatsoever, and that its meaning is not to be caught within the net of any pat formula. In the context of the proof, as in the living religions, the meaning and significance of the name of the Deity derives from a relationship to the sphere of man's religious experience.

I wish also to observe that the proposed revision of the ontological proof not only secures it against the well-conceived objection advanced by St. Thomas Aquinas, but indeed is actually in close accord with St. Thomas' analysis of the problem.

> "No one can mentally admit the opposite of what is self-evident, as The Philosopher states concerning the first principles of demonstration (*Metaph.* 1005 b 11). But the opposite of the proposition *God is* can be mentally admitted: *The fool said in his heart, There is no God* (*Psalms* LIII, 1). Therefore, that God exists is not self-evident. . . . . . Now because we do not know the essence of God, the proposition [that he exists] is not self-evident to us, but needs to be demonstrated by things that are more known to us, though less known in their nature — namely, by His effects."[9]

St. Thomas thus rejects the ontological proof on the grounds that "God exists" is not analytic (i.e. "self-evident") since its denial is no contradiction, but an intelligible assertion. However, St. Thomas' analysis does not stop here, but probes more deeply into the concept of self-evidence.

> "A thing can be self-evident in either of two ways: on the one hand, self-evident *per se,* though not to us; on the other hand, self-evident in itself, and to us. A proposition is self-evident because the predicate is included in the essence of the subject: e.g., *Man is an animal,* for animality is contained in the essence of man. If, therefore, the essence of the predicate and subject be known to all, the proposition will be self-evident to all . . . If, however, there are some to whom the essence of the predicate and subject is unknown, the proposition will be self-evident *per se,* but not to those who do not know the meaning of the predicate and subject of the proposition. Therefore, it

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[9] *Summa Theologica,* I, ii, 2, 1.

> happens, as Boethius says, that there are some notions of the mind which are common and self-evident only to the learned . . . . Therefore I say that this proposition, *God exists,* is self-evident *per se,* for the predicate is in the subject, because God's essence *is* His own existence as will be shown below. But because we do not know the essence of God, the proposition is not self-evident to us."[10]

What St. Thomas is here asserting is that a sound understanding of the term "God" would in fact entail assent to the proposition that God exists, so that this proposition is self-evident *per se,* but that this proposition is none the less not self-evident *simpliciter* because not only is the term "God" not among the "common notions of which no one is ignorant", but indeed "we do not know the essence of God". To this position, the present recasting of the ontological argument requires solely the addition of the thesis that *some* men do have, on the basis of their religious experience, an understanding of this essence, which, albeit partial, can suffice to make the proposition that God exists self-evident *for them* (just as, according to Boethius, some propositions are self-evident only to "the learned"). The present revision of the ontological proof is therefore in rather close agreement with the analysis of St. Thomas.

V. A Shortcoming?

The revised "proof", not being a formal, logically valid proof, lacks the power of compelling rational assent. If a correct and logically strict proof of God's existence, from wholly unobjectionable premises, were forthcoming, and this is the claim of the ontological argument in its classical formulation (cf. our motto from St. Anselm), it would provide a great theological weapon. For it would mean that rational persons could no longer withhold assent to the proposition that God exists, and would thus be a decisive means of refuting the sceptic and converting the unbeliever. Such persuasive force is wholly lacking in the revised version of the ontological argument here proposed. Since it sets up religious experience as a prerequisite for the argument, it is eminently unsuited as a means of confounding the atheist, sceptic, and agnostic. Thus the argument in its revised form is capable of carrying persuasive force only for those who actually do not stand in need of persuasion. With the proposed revision, the ontological argument has wholly lost its status as a proof. If I *demonstrate* the existence of a prime greater than any designated integer that is given, I can charge a person who persists in denying the existence of a prime greater than 10^{10} with a lack of intelligence or logical acumen. But no such change can be brought against one, who, in the face of the present recasting of the ontological argument, continues to deny its conclusion. (The only charge that we lodge against him is that of a lack of religious insight and experience, a lack which *also* precludes him from a sound understanding of the word "God" as used by religious people.)

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[10] *Ibid.*

I must confess at once that I see no way of refuting such an objection to the revision on the grounds of a loss of demonstrative force. However, in point of fact it is not properly an *objection* at all, but an inevitable recognition of a fundamental epistemological fact. There is, and can be, no way of *demonstrating* existence and there is no escape from Hume's thesis that only experience can provide knowledge of matters of fact and existence. Religious knowledge must derive from religious experience. In religion, as in science, there is no viable alternative to empiricism, for God cannot be demonstrated by logic. Logical manipulations cannot successfully be employed for theological purposes as a surrogate or shortcut for religious experience.

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DISCUSSION

POPPERIAN CONFIRMATION AND THE PARADOX OF THE RAVENS

By D. Stove

(*a*) Mr. J. W. N. Watkins has recently[1] tried to use the well-known paradox of the ravens to support Popper's view that instantial confirmation cannot accredit or render "probable" a universal generalisation. I do not think he has succeeded.

Watkins sets the paradox out as follows.

"Each of the following propositions seems intuitively acceptable: (1) Observations of black ravens confirm 'All ravens are black' . . . (2) Observations of black shoes, white swans, etc. are neutral to 'All ravens are black' . . . (3) If observations confirm one formulation of a hypothesis they confirm any logically equivalent formulation."[2] Yet not all of these can be true, for (1) and (3) entail that non-black non-ravens confirm "All ravens are black", contrary to (2). Watkins points out, furthermore, that if those white swans had been non-white, those shoes non-black — i.e. if the evidence had been quite contrary to what it was — it would still have instantially confirmed "All ravens are black" by confirming "Everything is not raven or black".

Watkins then writes: "We thus have the outrageous situation where a mass of evidence confirms a universal hypothesis and disconfirms its existential denial although a mass of flatly *conflicting* evidence would equally confirm the hypothesis and disconfirm its denial. The conclusion must be that the colossal inflation of instantiating evidence generated by Hempel's unquestionable equivalence-condition [(3)] is utterly irrelevant for the genuine confirmation of a universal hypothesis. Since an empirical universal hypothesis is instantiated by every object whatever except those whose existence it prohibits, it follows that, for its empirical confirmation, instantiation counts for nothing, and that only attempted dis-instantiation, attempted falsification, matters."[3]

(*b*) Let us, then, substitute for (1) above another condition, (W.1.): "Observations which fail to falsify 'All ravens are black' confirm it."

But of course this does absolutely nothing to mitigate the paradox! For all those black shoes, white swans, non-black shoes, non-white swans, etc., re-appear as observations which fail to falsify "All ravens are black"; and evidence which is effortlessly accumulated, enormous in bulk, and conflicting, accrues just as much as before to this or any universal hypothesis.

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[1] "Between Analytic and Empirical", *Philosophy*, April, 1957. Except where indicated, all my references are to this article.

[2] p. 116.

[3] p. 116. Italics in text.

(*c*) But it will be said that I am neglecting the provision that the observations which fail to falsify the hypothesis are to be observations made in *tests* of the hypothesis, i.e. in deliberate attempts to falsify it.

This provision is meant to be all important[4]. With it, Watkins is obviously right in saying that the policy of accrediting universal hypotheses by attempted falsification evades the embarrassment of "paradoxical" confirmation. But whatever "tests" are, a corresponding provision will just as obviously salvage the policy of accrediting universal hypotheses by instantial confirmation. For certainly no one would attempt to falsify "All ravens are black" by observing swans, shoes, etc.; but neither would anyone attempt to instantially-confirm it by such observations.

Thus if we amend (W.1) to read "Observations which are made in tests of 'All ravens are black', and which fail to falsify that hypothesis, confirm it", we must in fairness amend the original (1) to read "Observations of black ravens which are made in tests of 'All ravens are black' confirm that hypothesis". And now both accounts of confirmation evade the paradox, superficially at least.

It should have been obvious that what corresponds to "failure in attempts to falsify" is "*success* in attempts to instantially-confirm", and that it is completely tendentious to compare instantial-confirmation-paradoxical-or-non-paradoxical with non-paradoxical-failures-to-falsify. Once we make the appropriate qualifications by means of "tests", "attempts", and the like, the paradox cannot favour one policy against the other. (If what I say in (*d*) below is right, neither policy really evades the paradox.)

(*d*) Until we notice the tendentiousness of this comparison, we are bound to be led by Watkins' argument to think that the degree to which an hypothesis is accredited by observation somehow depends on the amount of effort that has been expended upon it; or at least that there is some logical connection between there being evidence for an hypothesis and efforts having been made to test it. Now we know that this is false, and such a consequence should arouse suspicion about an argument which leads to it. But Watkins actually embraces this conclusion, for he maintains that in order to accredit a hypothesis evidence must be "observed by deliberate intention, and not, so to say, stumbled upon by mere good fortune"[5].

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[4] Watkins italicises "tests" on pp. 118 and 120.

This seems to me a perfect *reductio ad absurdum* of the denial of a logic of induction. We know, of course, that effort — in searching, in contriving experiments, in checking initial conditions, in persisting in all of these and more — is often *causally* necessary and sometimes *causally* sufficient for the getting of evidence. But to suggest that effort can be *logically* even necessary, let alone sufficient, for there being evidence is to replace the whole question of the principles of inductive or scientific evidence by a question of the history and psychology of individual scientists. The *attempts* that might be made to falsify or to instantially-confirm a hypothesis are quite irrelevant to the weight of the evidence, if any, resulting from such attempts. It is the observations that constitute the evidence, not the attempts to make those observations.

If nature were kinder to scientists than she is, crucial experiments would abound without human contrivance; if she were much less kind, all the ingenuity of scientists' attempts to accredit hypotheses would count for nothing. But the principles of evidence can lay claim to objectivity and generality only if they are logical and hence independent of what nature is like. When we speak of a particular test of an hypothesis as "severe", for example, we mean, surely, to say something about the mutual logical bearing (including probability-logical) of various statements: *viz.* that the joint probability of the hypothesis being false and the result predicted by the hypothesis being true is much lower than the probability of the corresponding conjunction prior to this test. Whether we find it hard or easy to discover or contrive tests that answer to this description is nothing whatever to the purpose.

If we were to accept Watkins' idea that observations count as evidence only when they are deliberately made, we should never be allowed to say, what we quite often do say, that new and weighty evidence for a hypothesis has turned up in some quarter where no one had thought of looking for it. Of course, this disadvantage will be minor if we are to be allowed in return to say that the hypothesis that all ravens are black cannot be refuted by non-black ravens merely "stumbled upon"!

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[5] "Confirmable and Influential Metaphysics", *Mind*, July 1958, p. 316.

KANT AND THE COSMOLOGICAL ARGUMENT

By Peter Remnant

It is frequently said that Kant has shown that the cosmological argument for the existence of God presupposes the validity of the ontological argument, to the embarrassment of all those who accept the validity of the former but deny the validity of the latter.

However, it has been recently maintained by several writers that Kant is guilty of a simple logical howler in the course of his demonstration — in the words of one of these writers, that Kant's reasoning "is a piece of sheer though, of course, unconscious sophistry"[1].

I shall begin by stating Kant's argument. Kant represents the cosmological argument as comprising two parts: first, the argument that since something exists therefore a necessary being exists; second, the argument that if anything is a necessary being then it must be an infinitely perfect being, that is, God. He does not, as Father Johnston says he does, attempt to reduce the cosmological argument to the ontological argument. What he does do is argue that the second part of the cosmological argument presupposes the ontological argument. I shall restrict myself to what Kant has to say in defence of this contention.

If we accept the first part of the cosmological argument as sound and draw the conclusion that a necessary being exists we must then show that this necessary being possesses the properties which theologians attribute to God. But, although the cosmological argument begins with an appeal to experience, it "uses this experience only for a single step in the argument, namely, to conclude the existence of a necessary being. What properties this being may have, the empirical premiss cannot tell us. Reason therefore abandons experience altogether, and endeavours to discover from mere concepts what properties an absolutely necessary being must have . . . "[2] Reason concludes that only an

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[1] T. A. Johnston, S. J., "A Note on Kant's Criticism of the Arguments for the Existence of God", this *Journal*, 21 (1943), p. 11. *Cf.* J. J. C. Smart, "The Existence of God", in Flew and MacIntyre, *New Essays in Philosophical Theology*, pp. 36-37. Can sophistry be unconscious? In view of the prevailing tone of Father Johnston's paper the concession must be taken for what it is worth.

[2] Kant, *Critique of Pure Reason*, A606, B634. All references are to the Kemp Smith translation. Father Johnston has some harsh things to say about the quality of contemporary scholarship, but it is curious to find anyone still relying, as he does, on the old Meiklejohn translation.

ens realissimum could be a necessary being. But to draw such a conclusion is, Kant says, tantamount to carrying out the ontological argument.

This he seeks to show by setting the matter out formally. We wish to prove that a necessary being, any necessary being, must be an *ens realissimum*. Now if it is the case, as we hope to be able to establish, that any necessary being will be an *ens realissimum,* then, converting *per accidens,* some *entia realissima* will be necessary beings. "But one *ens realissimum* is in no respect different from another, and what is true of *some* under this concept is true also of all." (A608, B636.) Therefore every *ens realissimum* must be a necessary being. This means that "the mere concept of the *ens realissimum* must carry with it the absolute necessity of that being; and this is precisely what the ontological proof has asserted . . . " (*Ibid.*)

It should be clearly noted that "A necessary being must be an *ens realissimum*" is not, according to Kant, the *conclusion* of the first part of the cosmological argument. It is what still remains to be proved after completion of the first part of the argument. Failure to grasp this point has led another writer to attribute to Kant the bizarre argument that, since the conclusion of the cosmological argument is the converse of the conclusion of the ontological argument and "since the truth or falsity of the convertend entails the truth or falsity of the converse, it follows that *a being which must exist is perfect* (the conclusion of the argument from contingency) is no truer than its convertend, *a perfect being must exist* (the conclusion of the ontological argument)."[3]

The cosmological argument then, according to Kant, presupposes the ontological argument in the sense that the cosmological argument depends for its final step on the principle that any necessary being must be a perfect being; this principle entails the proposition that any perfect being is a necessary being; and not only is the latter principle the conclusion of the ontological argument, but the ontological argument constitutes the only even plausible attempt so far to establish the truth of this proposition. It should be noted, however, that even given the conclusion of the ontological argument we cannot deduce from it the principle needed by the cosmological argument without the aid of the further principle that whatever is true of some necessary being is true of all. In these exalted realms it is difficult to know which principles are self-evident and which need proof.

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[3] Gerard Smith, S. J., *Natural Theology, Metaphysics II.* N. Y., Macmillan, 1951. p. 69. Father Smith does not raise any objection to this argument as such, but denies that the one conclusion is the converse of the other.

So much for Kant's demonstration. On what grounds do Father Johnston and Professor Smart reject it as fallacious? Here is Father Johnston's criticism:

> "When he [Kant] goes through the process of converting the proposition 'Necessary being is infinitely perfect being', he takes it for granted that it is *by this very proposition* that the existence of necessary being is established, entirely neglecting the fact that the existence of necessary being has already been established by the previous step in the argument. When we say 'Necessary being is infinitely perfect', we are speaking of a being already known to exist."[4]

This is the whole of what Father Johnston has to say with regard to the fallacy in Kant's reasoning; he does not say why he thinks that Kant is taking it for granted that the existence of necessary being is established by the proposition in question. But it would be curious if Kant had forgotten already what he had written on the previous page, where the problem is clearly stated — Given that we have proved the existence of necessary being, how can we prove that this is the *ens realissimum?* — and particularly curious since, as Smart points out, the conversion of "Any necessary being is infinitely perfect" to "Some perfect being is necessary" is valid only if a necessary being exists.

Smart's criticism adds nothing substantial to that of Father Johnston: Kant has forgotten that the existence of a necessary being has already been proved; he has been taken in by the ambiguity of propositions of the form "All As are Bs", which may mean either "There are As, and they are all Bs", or "If there are any As then they are all Bs"; Kant "has taken himself and other people in by using 'all' sometimes in the one way and sometimes in the other".[5]

I can see no force whatever in these criticisms — there is no reason to believe that Kant has forgotten or been taken in by anything in the course of his demonstration. Granting for the sake of argument that the first part of the cosmological argument has proved the existence of a necessary being he then asks: "How can it be proved that a being of this kind must be supremely perfect?" and he replies: "To prove that a necessary being must be a perfect being amounts to proving that a perfect being must be a necessary being — and this is just what the ontological argument, and no other known argument, is intended to do." It is irrelevant that in this case we already know that there is a necessary being — the ontological argument is not designed to prove *simpliciter* that there is a necessary being, and it is not assisted in arriving at its own proper conclusion by the knowledge that there is a necessary being.

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[4] Johnston, *op. cit.*, p. 15. Italics in the original.

[5] Smart, *op cit.*, p. 37.

That this is the line of Kant's reasoning is borne out quite clearly by what he says in his introductory remarks to the discussion of the three theological arguments:

> " . . . If we take the issue as being that which is here stated, namely, *first,* that from any given existence . . . we can correctly infer the existence of an unconditionally necessary being; *secondly,* that we must regard a being which contains all reality . . . as being absolutely unconditioned, and that in this concept of an *ens realissimum* we have therefore found the concept of a thing to which we can also ascribe absolute necessity — granting all this, it by no means follows that the concept of a limited being . . . is for that reason incompatible with absolute necessity." (A588, B616.)[6]

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[6] Kemp Smith gives "reality" as the last word of the quotation. This is obviously a slip on his part—the German reads "Notwendigkeit".

CRITICAL NOTICE

The Illusion of the Epoch. By H. B. Acton. London: Cohen & West, 1955. 278 pp. 18s. net (U.K.)

Professor Acton has not succeeded in justifying his conclusion that "Marxism is a philosophical farrago" or, in general, in throwing fresh light on Marxist doctrine. A study which would bring out the real merits, as well as the real difficulties, of Marx's position would not follow the line indicated by Acton's sub-title ("Marxism-Leninism as a Philosophical Creed") and treat secondary figures like Engels, Lenin and Stalin (!) as clearing up matters which Marx leaves doubtful; nor would it confront Marx with merely commonplace views of "freedom", "right", "the individual", "the external world" and so on (all the bad pennies of philosophy) instead of exhibiting in detail Marx's case for the "illusory" character of such conceptions. A great deal of work is needed for this, Marx's directly philosophical expositions being neither extensive nor free from confusion; but to throw in the much greater confusions of his successors is no substitute for the positive and illuminating doctrines which a critical re-statement of his arguments can bring out. The "illusion" Acton refers to appears to be that of wholesale planning (human life guided and controlled down to the last detail) to which Marxism is in no way committed; his own illusions, unshaken by the impact of Marxism, are those of a vague "social betterment" and of a "speculation", a rumination on "first principles", which is not merely unhistorical but anti-historical. It is this "starting from scratch", characteristic alike of present-day Communists and of their opponents, which might properly be called "the illusion of the epoch".

Lenin, of course, in *Materialism and Empirio-Criticism,* was not setting up as a philosopher; his point was that even a non-philosopher could show that the various "empirical" views which were being canvassed in the party were in conflict with the doctrines of Engels and therefore with a true revolutionary line. His non-expertness comes out both in his hammering on the crude "copy" theory of knowledge and in his constant recourse to accusations of "fideism" (or deviation into *religious* views). But such crudities give no ground for saying that Marxism as such has no adequate reply to "phenomenalism" or for taking it to have a close affinity with "positivism" — as Acton does largely on the strength of its being anti-religious. It is true that Marxism is positivist in the sense of denying all "higher realities", but what is particularly being denied is anything of a supra-historical character, whether "religious" or otherwise, and, in viewing reality as historical, Marxism has little in common with any of the doctrines that have been called "positivism".

Again, Acton shows no awareness of the quite pedestrian character of Stalin's thinking and, while recognising that he also echoed Engels, seems to regard him as a thinker of considerable originality (apparently even, on p. 156, crediting him with the notion of large-scale industry) and treats as important (p. 260) his crude observations on language. "A language, he argued, must be the same for a whole society, for proletarians as well as for bourgeoisie, if communication is to be possible and the society is to hold together. Whereas the change from feudalism to capitalism and from capitalism to socialism involved sudden breaks or leaps, language develops gradually and in independence of such changes in the economic basis, although, of course, the vocabulary is affected by them." Such a view (which Acton takes to show the inadequacy of the distinction between "basis" and "superstructure" in social life) implies no appreciation at all of the Marxist and historical view of things according to which language must develop as a feature of social activities and must vary with different ways of life, just as it is broadly recognised to do between the "learned" and the "vulgar" (one of the many points ignored by the contemporary partisans of "usage"). It should be understood, of course, that classes for Marx do have communication with one another and are not, as Acton in various places seems to imply, in conflict on all points (are not separate "nations"); but still they have a diversity of outlooks and hence of modes of expression, the extent of their communication is limited, even if they are roughly said to speak "the same language" and are parts of what is called a single society. In fact, if one were looking for something "socially neutral", something cutting across diverse forms of social activity and organisation, language would be a particularly bad example to choose. To do so would be to reduce it to an instrument of commerce or, at most, of production, instead of recognising it as a repository of the history, of the social movements, of its users. But, even on an instrumental view of it, it could not be treated independently of "the economic basis of society" or of economic organisation generally.

For Marx, then, any question of human affairs is to be considered in terms of types of activity or ways of living, and a theory of knowledge in particular will have content only as a theory of interrelated activities. And this makes Acton's long-drawn-out discussion of "phenomenalism" beside the point. Even if Engels replaces the Hegelian conception of "reflection" by a Lockian representationism (as Lenin clearly does), this is alien to Marxism and to a *historical* view of knowledge — a view which permits learning or the acquisition of knowledge. Certainly Engels cannot show how "practice" can break down the barrier between inner and outer, if once this division has been set up. But the point is that Marx denies the division; the position he puts forward in the first of the *Theses on Feuerbach,* particularly, is that human sensory activity is "the active side" of the sensuous or, in a more recent terminology, that the object is the content of the mental process. There are obvious difficulties in this — Marx indeed seems to infer, from the fact that knower and known are both activities, that they are the same activity — but, without drawing this inference, we can still say that their being both activities enables them to be continuous with one another, with no intervening barrier, and that this continuity, the way in which mental and non-mental processes can be embraced in a single process or can "belong together", is the positive content of the doctrine of "reflection" and is explicitly set against any doctrine of mere *confrontation* (or of mere "data") which would make knowing impossible. Marx's position here is also opposed to the contention of Lenin that matter has a primary and mind a secondary reality, since mental processes are material (in the sense, not of being composed of some common substance, which is not true even of different non-mental things, but of being *space-filling* — apart from which the continuity referred to would not exist); though there is nothing in this materialist position incompatible with the view that, in any region in which mental processes exist, non-mental processes have *preceded* them. The inability to escape "phenomenalism", then, does not apply to Marx, since he is at the materialist standpoint (is raising questions of actual processes) at all stages.

The general position of the *Theses* is that to have true knowledge is to be moving with the movement of things, which is a revolutionary movement; it is only the revolutionary, participating in that movement, who really understands it — who has a "dialectic" understanding (i.e., precisely *participatory,* going beyond himself) as contrasted with the idle speculation of the non-participant. Apart from the major difficulty of the unification of all movements (something which is really incompatible with a historical logic or philosophy of process), there is the minor difficulty of showing how mere speculation, as non-involvement in reality, is even possible. But it is something quite different (and quite indefensible) to treat Marx as rejecting "speculation" in the sense of the inquiry into reality and demanding a concentration on "science" in the sense of the formulation and search for verification of hypotheses in some particular field. It is easy enough to pick texts which appear to be anti-philosophical, but a person who calls his doctrine "material*ism*" is certainly not repudiating philosophy. What Acton is doing is reading his own view of science, and his own view of "formal logic and dialectic", into Marxist doctrine. The position is not that "the laws of logic somehow copy or 'reflect' the real world" (p. 104) — or again that first principles or "fundamental notions" can be considered apart from concrete inquiries and activities; anything that exists (mental process as much as non-mental) is in "the real world" and there is no "external world" or "material world" from which some other region could be distinguished and set apart. The scientist is grappling with the only reality there is, and since it is dialectic or revolutionary he has to be a logician and dialectician — in other words, a philosopher. There is much to be said in criticism of the *Theses* — especially with regard to the line of unification in which what environs society is treated as itself social, so that all reality is social reality, all movement the social revolution — and in bringing out, in the general "materialist" position, the oscillation of "material" between the meanings of *substantial* (being composed of the old "universal stuff) and *social* or, more exactly, *productive,* constituted by co-operative activity. But there is nothing in all this to suggest that Marx took a narrow procedural or instrumentalist view of science.

Even if the method of the "natural sciences" were properly described as that of "hypothesis, observation and experiment" (p. 58), it would not be apparent how this was to be distinguished from the method of any other group of sciences; but, beyond that, it would not appear either how hypotheses came to be framed or how the procedure could break through to anything non-suppositional and thus be accorded "efficacy". If, in Marx and Engels, the test of "practice" is especially that of "touching and manipulating" (p. 50), this does not mean that tactual "data" are being given a special position, being taken to "break the barrier" in a way that other "data" do not; the point still is that there are transactions (contacts) not with "data" but with actualities, that there is no "judgment" or "interpretation" distinguishable from "data" in any field, and that without "manipulations" there can be no acquisition of knowledge, so that there can be no science distinct from "physical" science. It is not merely that in any knowledge there must be continuous processes embracing knower and known, but that there could be no connections between different *fields* (no possible attribution, e.g., if ethics were not a physical science, of an ethical character to anything that had a physical character) but simply separate "universes" or "worlds", each with its quite separate *knowers*. If, however, there *is* knowledge by the same knower of human and non-human facts, there must be continuity among them and they must all alike be historical or "physical"; as against the insoluble problem of relations between different kinds of reality, there is no difficulty at all about historical relations between historical things. This is the general point that "historical materialism" makes, and Acton does nothing in his criticism to show how the problem of discontinuity can be dealt with. Rather, he simply assumes that there can be facts of a lower or derivative kind (having the status of mere resultants) and facts of a higher kind (objects of "judgment", "values"): and this means, in particular, that for him there are no distinctively *social* facts and that he can only grope for Marx's meaning, can only give a series of inept *individualistic* interpretations of Marxian doctrine and find *it* incoherent because they are inadequate. That its very thesis is that coherence rests on the historical character of things, on the commensurability and interrelatedness of social, psychological and any other processes, would never appear from Acton's pages.

There are still, of course, obscurities and confusions in Marx's presentation of his view; in particular, if what can positively be made of "historical materialism" is to be brought out, a certain ambiguity in "historical" as well as in "material" must be noted. A historical view of things can be understood as a doctrine of reality as process, and this, as we have seen, is part of Marx's meaning. But there is also the more special meaning of "history" as *social* processes. And the "materialist" interpretation of history, in this sense, is the doctrine that productive organisation is what distinguishes social from other processes, is the "subject" of history or that which continues (though having distinguishable phases) while a society continues. It is in accordance with the Hegelian foundation of the doctrine of "subject" that social history should be identified with a postulated movement of things in general and that all its characters should be taken as embraced in a single conception — but such "totalism", as against pluralism (recognition of the distinction of any process from surrounding processes and of an irreducible plurality of processes within it) cuts at the root of concrete continuity and leads to forced interpretations and distortions. But it is still the case that only with the recognition of specific types of social activities and interactions can there be any social theory or any account of "history". And even if Marx confuses this particular subject with reality in general, he does make positive contributions to it — while at the same time showing us that such contributions can be made only if a philosophy of process is maintained and such unhistorical ("metaphysical") entities as "the ego" or "freedom" are discarded.

Mental and social activities, then, are processes among others, which they affect and are affected by, having the same status, the same regularity and predictability. But there is no question of *total* prediction or of a single "course of history". It is just because a strictly materialist view is pluralist as well as determinist that Marx's doctrine of single-track social development is a departure from it and forces him into such further inconsistencies as the doctrine of "shortening the birthpangs". The activity of a revolutionary party is in the same condition of interchange (regular transactions) with its surroundings as any other process, and if there were a single course of social activity, the party activity would have to be *part* of it — to be seen in its place within an inevitable progression — and could not come from outside and expedite it. But to note such inconsistencies is not to answer a materialism of a non-totalistic kind, of which Marx laid the foundation in his doctrine of social struggles throughout history. Thus when Acton says (p. 185) that we "deceive ourselves with almost empty phrases if we suppose that we can make predictions about such things as societies, civilisations, revolutions, classes, social orders, and constitutions, as we can about men, genes, gases, and stars", he is not showing how in that case we can talk about the former at all or what possible relation we can take them to have to the latter if they do not have the same kind of reality, and he is passing over or denying the fact that we can talk about a man, e.g., only as a complex of interrelated processes (a subject of internal and external "exchanges") and is blind to the fact that there is no greater illusion (no greater "emptiness") in the modern period than that of the "ego" standing above and directing, instead of *being,* processes. It is just this sort of thing that Marx is specially attacking in the *Theses,* which are an indictment of modern (Cartesian, egoistic) philosophy, and in the whole materialist conception of history.

There is no question, then, of social processes having a reality inferior to that of psychological processes; indeed, if we are to give any serious account of a man's life, we must present the interrelation (including the clash) of forms of activity in which he and others participate — of which he is a "vehicle" rather than an initiator. It is, for Marx, the development and sequence of forms of *productive* activity in particular that is social history (history in the usual sense), and this is only caricatured, and incidentally in an individualist way, in Engel's assertion that man must eat and drink, be clothed and sheltered, before he can engage in other activities. It would be tedious to go over the points at which, in his chapters on "Historical Materialism" and "Marxist Ethics", Acton comments on Marx in individualist terms and turns social questions into questions of someone's demands. But, to take one example, when he says (p. 112) that a "man's needs may be understood in the sense of everything he desires" (following this up, on p. 113, with the extraordinary statement that "we generally take it for granted that social science should find means for satisfying, not any and every desire, but *legitimate* desires"), he is missing the point that "needs" for materialism are what are required to keep certain things going and that these things are forms of activity, not "individuals". It is only in this way that there can be concrete questions, capable of being investigated, and thus that there can be social science — and not the loose guessing which is all that contemplation of X's choices, Y's choices, etc., can give rise to. In the same way, to Acton's observation (p. 155) that "the individuals who make up society do not get fused or transmuted, as in chemical combination", the answer is that forms of activity do get fused and transmuted, and so far from there being social atoms who can merely form a "mechanical mixture", it is such forms of activity that make up "individuals".

Illustration of this and connected points is obtained in Acton's treatment of "relations of production". He supports, as others have done, the view that the materialist interpretation of history is a technological interpretation, an explanation by reference to instruments of production — and, in this connection, that by reference to which an epoch is to be understood (its "basis") is taken as its cause or that which brings it about. The "causal" way of speaking is, of course, an easy one to slip into, and Marx also speaks occasionally as if his doctrine were a technological one ("the windmill gave us feudalism, the steammill capitalism", etc.); but, if such formulations were to be taken literally, Marx's whole distinction between "productive forces" and "relations of production" would be done away with. Acton speaks of the latter (p. 135) as "the ways in which men are related to one another as they operate the 'productive forces' ", which suggests that it is simply a question of the interrelation of special operations within a general operation, of a complex manner in which energies are directed on materials — and such technical devices would still come under the head of "productive forces". As against any such differentiation of tasks within a single labouring group, what distinguishes "relations of production" is differentiation of *classes,* ways in which the different *factors* in production (land, capital, organisation, and not merely labour) are appropriated and made available for productive work, and secure their different types of return (rent, interest, profit, as well as wages). We find, of course, in different economies different combinations of these types of appropriation, but we certainly cannot describe any economy unless we take account of the forms of social procedure involved (whether or not we use expressions like "exploitation" in describing them), and thus unless we distinguish "relations of production" from "productive forces", however we take them to be connected. But, if classes are thought of merely as aggregates of "individuals", then it is natural to revert to ordinary "division of labour" and lose the major point.

Again, on p. 167, Acton says that his analysis of the notion of 'productive relationship' "has shown that this involves law, morals, and politics, and we can see that it is not fanciful to regard them as parts of the means of production". The former of these points is not denied but asserted by Marx; he is saying, in particular, that it is to a given factor in production that "rights" belong, that it is their "subject", that through which alone their character and history can be grasped, just as the productive process in general is the "subject" of the whole system or distribution of "rights". But the bearer of "rights" is still the function, the *provision* (which can be accomplished in various ways) of a certain factor to production, and not the provider as a separate "individual". And again it is not a question of the function's preceding the "rights" in time; there can be no system of appropriation of factors without a system of sanctions, however primitive, but Marx's point still is that it is from the former that the latter can be understood, and especially that there can be no concrete and historical account of law except by reference to its economic "basis", no intelligible treatment of it as something independent or "absolute", as carrying its own "principles" with it. No doubt it can be argued that

Marx takes too narrow a view of productive activity, that social creativeness cannot be reduced to industry (and emphatically, of course, cannot be understood as a mere matter of survival), but still the search for the continuing "subjects" of social processes is a condition of determining their regularities (laws) and of having an objective social theory. This cannot be obtained on the level of conceptions like "performing one's task" or "man, the tool-using animal" (these precise phrases do not occur in the text), but when he reduces the class theory of "relations of production" to a doctrine of the personal use of means of production, Acton is paring away all that sets Marx above that level.

Just as it is his individualism that prevents Acton from getting hold of the materialist view of society, so it is his moralism that prevents his grasping the materialist view of ethics. "The adjective 'moralistic' ", he says (p. 213) "is today used in spoken English to express criticism of exaggerated or misplaced moral judgments . . . More generally, the noun 'moralism' is used for an exaggerated or misplaced zeal for conventional moral rules." The materialist objection, however, is on the ground not of exaggeration but of error; it is a direct rejection of doctrines of "right" or "ought to be". No doubt Marxists are sometimes inconsistent here as on the question of freedom and can be found taking a preceptual line; but their main position still is that moral or ethical questions are questions of concrete social activity and that the notion of right (or required) conduct is misleading when, as it commonly is, it is put forward without specification of the activity for which it is required, the sort of life which the conduct keeps going — and that the explicit view that no question of such specification arises, that certain things are just "right", is a thoroughly confused one. This is not in the least to say that ethical activities of this or that kind cannot be investigated as much as anything else; it is only to say that such investigation is just as positive, just as much an inquiry into fact, as any other. It may be (p. 178) "commonly supposed that one important characteristic of the method of the natural sciences is to be free from any preconceptions about the value, the goodness or badness, the perfection or defect, of what is being investigated", or, more briefly, that natural science is "value-free". But the Marxian contention is that either goodness and badness are the subject of a natural science or there is no knowledge of them at all, that either "values" are natural facts or there are no "values". As previously argued, there will be no assertable connection between objects of observation and objects of "judgment" unless they can be taken in a single view, as parts of a single experience; and this means that that "judgment" which is supposed to be beyond the compass of natural science does not exist. There is no subject or argument in which "the moral beliefs of men", as contrasted with the occurrence of the objects of these beliefs, "are taken as data" (p. 66); the question is what concretely is believed or found to be fact, and to this question, or this line of Marxian attack, on "conventional moral rules" Acton has simply not addressed himself.

What he sets against Marxism is an "ethics" as empty as the doctrine of "the individual". This is illustrated on p. 184 by the notion of "physical evils", which can only mean certain physical things people want to avoid and which is paralleled by the notion of "goods" as objects of desire, neither notion *characterising* the objects in question. If there is anything that has an ethical character of its own, then the question of its nature and history can be gone into, and ethics will be the positive science of ethical processes(more particularly, good activities) — it will be quite a side-issue whether anyone wants them or not, and there will be no question of the pursuit of them being "absolutely required" of anyone or of their being among "the principal purposes of human life" (p. 1). And, however incomplete Marx's account of the going on of such processes may be, it is no reproach or ground of objection to his position that he still recognises positive goodness while rejecting "moralism" or the "ethics" of categorical imperatives; while at the same time any deviations into moralism on his part do not affect the strength of his "materialist" view. It is those who take ethical assertions as something other than the assertion of certain historical occurrences who are really jettisoning the science of ethics.

The doctrine of absolute "right" (of that which *in itself* is required or obligatory, i.e. of a relation lacking its full complement of terms) is an outstanding example of what Marx and Engels call "ideology". This is a question which Acton handles weakly, even taking as a serious development of theory the use of "ideology", by Stalin and others, in the commonplace sense of "ideas" or beliefs. Acton quotes (p. 126) the passage in which Marx and Engels say, "If in all ideology men and their circumstances appear upside down as in a *camera obscura,* this phenomenon arises just as much from their historical life-process as the inversion of objects in the retina does from their physical life-process", but, while taking this to assert that it is natural that men should get distorted views of "the world", he does not appear to grasp the force of *inversion*. The point is that A is taken to depend on B, when in fact B depends on A — and thus, not only that a certain line of conduct is taken to be *in itself* right or required when in fact it is required for the carrying on of certain social activities or the maintaining of certain social relations, but that it is taken to be what sanctions, or lays down the necessity of, these activities and relations. The "illusion of the epoch", then, is that such "sanctions" are absolute or primary instead of being relative or secondary (being incidental to "capitalist production", e.g.) ; and, while it is positive inquiry, and not something that is itself "ideology", that establishes such facts of derivation — that completes the formulae, misleading in themselves, by adding "while capitalism exists" or "if capitalism is to be maintained", etc. — it is clear that such inquiry carries with it a critical apparatus, that it involves a philosophical examination of forms and is not a mere collection of "data"; and this reinforces the point that Marx's positive doctrines cannot be separated from his philosophy, even if much that is called philosophy would be described by him as ideology.

The doctrine of "data", indeed, is just another example of ideological or "relativist" thinking — the postulation of "that whose nature it is to be given to us" (or that which has *givenness* in itself) is on a par with the postulation of "that whose nature it is to be required of us" (or to be obligatory), and the rejection of it exemplifies rejection of "natures" and the insistence on explanation in terms of concrete activities, of the interaction of processes all on the same empirical level. As before, Marx's "totalism" prevents him from bringing out this form of argument clearly and in detail, but it can still be said that this is the positive content of his doctrine of "ideology" in the same sense as that in which he himself exhibits the positive content of doctrines which profess to be independent of and indeed to stand above social processes. The vital consideration is still that of continuity, and it is this that Marx is upholding in his "materialism". In doing so, whatever his incidental errors, he has contributed notably to the philosophical criticism of dualism or the setting up of different kinds of reality, individualism or the postulation of any sort of atomic entity, and relativism, or the doctrine of constitutive relations, which is only one example of rationalism or the doctrine of "natures" as opposed to transactions, alike within anything and between it and other things.

These are the large issues that arise, but Acton is so fixed in his "separatism" — separation of "individuals", separation of kinds and "methods" of inquiry so that no connected view of things is possible — that he cannot arrive at a general view even of *Marx's* position but engages in piecemeal and pin-pricking criticisms. It is, I would maintain, the dualistic or divisive view that is a "farrago" instead of a philosophy. At any rate, it is the question whether there can be any "division in reality", especially whether anything unhistorical can be set against the historical, that specially calls for consideration which it does not receive in this book. Perhaps the main barrier to such unfettered discussion is Acton's inability even to conceive the possibility of the rejection as illusory of an "ethics" of right, carrying with it a moralistic treatment of all human affairs.

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REVIEWS

The Uses of Argument. By Stephen Toulmin. Cambridge University Press, 1958. vii, 264 p. 22s. 6d. (U.K.)

(1) *Summary*

This book is described by its author as radical in intention but unoriginal in the arguments that it contains. It consists of five essays which are meant jointly to support the thesis that logic and epistemology should be replaced by a study which might be called comparative logic.

In the first chapter we are told that two arguments are in the same field if and only if the data of the two arguments are of the same logical type, and the conclusions are of the same logical type. Some features of arguments are field-invariant and some are field-dependent: the force of modal qualifiers such as "necessary", for example, is the same in all contexts, although the criteria for their use vary from field to field.

The second essay, which is on probability, is self-contained, but it is presented, together with the first, as being in part a preparation for the third essay in which "the crucial results of the inquiry are expounded". (p. 8.) Here it is contended that the traditional analysis of arguments has been crude, and a new layout, designed to reveal details previously obscured, is proposed. This leads to a distinction between analytic and substantial arguments.

In the fourth essay it is maintained that logicians, believing that logic might become a formal science like geometry, have taken as common and typical analytic arguments which are in fact most unusual, and that they have consequently lost touch with the business of assessing the substantial arguments that we use from day to day. In the final essay the philosophical consequences of this alleged error are sketched. In substantial arguments we often make type-jumps: we argue from the past to the future; from other people's behaviour to their feelings, or from what is to what ought to be. In such cases our arguments cannot be analytic, and if we identify soundness of argument with analyticity, we have to conclude that our arguments cannot be sound. Thus we come to believe that we never can argue cogently from the past to the future, or from behaviour to feelings, and we reach a state of puzzlement which is alleviated only when we see that substantial arguments may be sound although they are not analytic.

The criteria for the validity of substantial arguments vary from field to field and consequently the investigation of validity and other logical concepts will be a comparative study. Epistemology is to be divided between psychology and this new inquiry, and formal logic will remain beside the point. This last chapter and the one on probability are provocative, but, as the title of the book suggests, and as Toulmin says, it is the analysis of arguments that is central. In this notice, my remarks will be confined to this central topic.

(2) *The New Layout*

Toulmin does not claim that his discussion of arguments is exhaustive. He chooses to investigate the type of argument that can be put into the form: All X's are Y's, and S is an X. So S is a Y.

An argument is put forward to justify or to defend a claim, and it is by considering the lines of defence to which we retire when defending a claim that Toulmin gives his analysis of arguments. If we make the claim (C) that Harry is a British subject, we may be asked for the information upon which this claim is based. In reply, we offer the datum (D) that Harry was born in Bermuda. The connection may not be obvious, and for this or some other reason we may produce the warrant or inference licence (W) which justifies the step from D to C: in this case "A man born in Bermuda is a British subject".

A warrant may not justify the step from data to conclusion without qualification. "If a child has no brothers or sisters, it is likely to be selfish" does not warrant our passing from the datum "Harry is an only child" to the unqualified conclusion "Harry is selfish". In this case the conclusion that Harry is selfish must be qualified by the expression "it is likely that". Toulmin calls such expressions modal qualifiers (Q).

It may also be the case that a warrant is subject to exceptions. Given that Harry was born in Bermuda, we may argue that since a man born in Bermuda is *generally* a British sbject, Harry is *presumably* a British subject, *unless,* of course, he has become a naturalised American. Here "presumably" is a modal qualifier, and the "unless" clause is a statement of conditions of rebuttal (R).

At this stage our critic may accept that our warrant justifies our basing a suitably qualified claim upon our data, but he may question the warrant. In this case we produce the information upon which the warrant is based. This Toulmin calls the backing (B) for the warrant.

The simplest form of argument is "D, therefore C" or "D, so C", the C often being qualified in some way (Q). Unless we are challenged, or anticipate a challenge, the warrant (W) remains implicit. If W, having been stated, is questioned, we produce the backing (B). A line may be drawn here, for B is a factual statement, and, if challenged, it becomes a claim in defence of which we may produce another argument.

(3) *Modal Qualifiers*

Some of the modal qualifiers that Toulmin mentions or uses are "necessarily", "possibly", "probably", "presumably", "certainly" and "almost certainly". In one place he describes them as indicating "the degree of force which our data confer on our claim in virtue of our warrant" (p. 101), while in another he says that they indicate "the strength conferred by the warrant on (the) step" from data to claim. (p.101.) If we introduce the modal qualifier "necessarily" into an argument of the form

Since p, it follows that X is Y

we get either

(i) Since p, it follows that X is necessarily Y

or (ii) Since p, it follows necessarily that X is Y

depending upon which of Toulmin's two statements we follow. That this confusion is not trivial is indicated by the fact that there are values for "p", "X" and "Y" such that the same substitution in (i) and (ii) gives two different arguments, only one of which is valid.

My second point is that if we consider the modal qualifier "certainly" instead of "necessarily", then my first point cannot be made. "Since p, it follows that X is certainly Y" differs only in emphasis from "Since p, it certainly follows that X is Y". From this it appears that Toulmin has assembled under one heading expressions that are different from each other in a way that is important in logic: a conclusion that is confirmed by comparing "probably" with "necessarily" and with "certainly".

(4) *Form and Validity*

It is suggested that the difference between a major and a minor premise is so great that to call them both premises is scarcely justifiable. The claim or conclusion of an argument of the type under examination is a categorical statement of fact, and so is the datum or minor premise. The backing for a warrant will also be categorical, but the warrant itself is not. The warrant or major premise is hypothetical and general. "It is more than a repetition of the facts (of which the backing consists): it is a general moral of a practical character about the ways in which we can safely argue in view of these facts." (p. 106.) The nature of the backing for a warrant, and the relationship between it and the warrant, vary from field to field. "The whale is a mammal", for example, is backed by a system of classification; "Anyone born in Bermuda is British" by statute, and Kepler's Laws by astronomical observations. Inasmuch as all these warrants are warrants, each of them has the force of a general hypothetical. But to go on to say that statements of the forms "The S is a P", "Any S is P", "S's are P's", and so on, are all really of the same form, is to ignore how, by our choice of words, we indicate the nature of the backing for a warrant. If we reduce all warrants to one or two forms, we obscure the diversity of their backings, and this we should not do because it is the backing that licenses an inference, the warrant merely showing what sort of inferences are licensed by the backing.

Sometimes one and the same sentence may be used to state a warrant, or to state the facts that back the warrant. Suppose we have an argument of the form: All X's are Y's, S is an X, so S is a Y, and suppose that the major premise has been established by complete enumeration. We may paraphrase the major premise in a way that emphasises its function as a warrant, or in a way that brings out its factual content. In the first case we have

(w) An X is certainly a Y
S is an X
So, S is certainly a Y

and in the second

(b) 100% of the X's are Y's
S is an X
So, S is certainly a Y.

Toulmin now says that when the argument is in form (w) "the parts of the conclusion are manifestly the same as the parts of the premises, and the conclusion can be obtained simply by shuffling the parts of the premises and re-arranging them" (p. 119.) This is true, but it is not the case, as Toulmin appears to suggest, that this is what is meant by saying that the argument is formally valid. Later on, he describes a warrant as being, in effect, a substitution rule (p. 210), but this is not sufficient either. These objections may seem pedantic: it might be replied that it would be easy for Toulmin to work out and to state which formal moves are permissible in arguing and which are not. This may be so, but to work this out would be to do some formal logic, and this activity is dismissed as being beside the point.

According to Toulmin any argument from D to C can be made formally valid by introducing a suitable warrant, but the step from D to C will be sound only if the warrant is properly established. The validity of an argument depends upon the warrant's being established, and its conclusiveness depends upon this and upon the truth of the data. The warrant can be established only if it has the proper backing, and consequently it is maintained that "once we bring into the open the backing on which (in the last resort) the soundness of our arguments depends, the suggestion that validity is to be explained in terms of 'formal properties' in any geometrical sense loses it plausibility" (p. 120). It is not that the formal properties of an argument justify the step from the premises to the conclusion, but rather that the appropriate established warrant justifies the step from data to claim. Toulmin is right in emphasising the very different functions of major and minor premises in arguments of the type in question, but he is wrong in supposing that formal considerations are of no importance. He says that the validity of an argument cannot depend upon the formal features that are apparent when the argument is put into form (w) because the argument remains valid, or invalid, as the case may be, when it is put into the quite different form (b). But this is a *non sequitur*.

Three things are severally necessary and conjointly sufficient for the conclusiveness of an argument of the type that Toulmin is considering:

(p) that the warrant shall be formally appropriate,
(q) that this warrant shall be properly established,
(r) that the datum shall be true.

To these we may add a condition necessary for (q) and thus indirectly for the conclusiveness of the argument:

(q') that there shall be backing for the warrant. Toulmin talks as if (q') were a sufficient condition, and as if (p) were trivial, even though it may, strictly speaking, be necessary. "Provided that the correct warrant is employed", he says, "*any* argument can . . . become formally valid" (p. 119; my italics). But to investigate systematically which warrants will render a given argument formally valid is to do formal logic. Whether the warrants that have been established warrant the steps that we wish to take is a formal question, but answering it is not a mere formality.

(5) *Analytic and Substantial Arguments*

Consider the following arguments:

(iii) Petersen is a Swede. The proportion of Swedes who are Roman Catholics is zero. So Petersen is not a Roman Catholic.

(iv) This statue is made of bronze, and is, of course, exposed to the air. Bronze things eventually turn green if exposed to the air. So this statue will turn green.

Whoever established that the proportion of Swedes who are Catholics is zero, will have checked Petersen's religion, and thus in (iii) to check the backing is *ipso facto* to check the conclusion. In (iv) however, the backing for the warrant does not — indeed cannot — include the conclusion. Toulmin calls (iii) an analytic argument and (iv) a substantial one.

The discussion which leads to a definition of "analytic" is hard to follow. There are two different things that Toulmin might be doing here. He might be investigating the relations between an established concept of analyticity on the one hand, and verification, self-evidence and tautologousness on the other. Or he might be giving a stipulative definition of a new sense of "analytic", and pointing out that the distinction between analytic and substantial arguments to which it gives rise is not the same as a distinction based upon tautologousness or self-evidence. The definition at which he finally arrives makes it clear that he is introducing a new sense of "analytic", but he confuses the discussion by proceeding as if he were examining an established concept. He also confuses the grammatical sense of "tautology" with the logical sense of this word.

Let us consider an argument in which a modal qualifier is used.

(v) Petersen is a Swede. The proportion of Swedes who are Roman Catholics is five per cent. So it is almost certain that Petersen is not a Roman Catholic.

If we take the conclusion of this argument to be "it is almost certain that Petersen is not a Roman Catholic", then we have to explain how such a conclusion may be verified. This question does not arise for Toulmin, because for him "it is almost certain that

Petersen is not a Roman Catholic" is not a statement about the statement "Petersen is not a Roman Catholic". To say "it is almost certain that p" is to assert "p" in a guarded way. Whether this is right or wrong, it commits Toulmin to saying that the conclusion of (v) is "Petersen is not a Roman Catholic". The first consequence of this is that (v) is analytic since to check the backing would be to check the conclusion. The second consequence is that the premises of (v) may well be true and the conclusion false, and this despite the fact that (v) is by normal standards a sound argument. Toulmin accepts this and decides to "class an argument as analytic if and only if . . . checking the backing of the warrant involves *ipso facto* checking the truth or falsity of the conclusion . . . we shall do this" he says, "whether a knowledge of the full backing would in fact verify the conclusion or falsify it." (p. 133.) It follows from this that if we negate the conclusion of an analytic argument we obtain another analytic argument. Toulmin does actually say that analyticity, like formal validity, is not a criterion of soundness in an argument (p. 145), but sometimes he forgets how he has decided to use "analytic", as when he writes "In the analytic syllogism, the conclusion must in the nature of the case repeat in other words something already implicit in the data and backing; . . . " (p. 151).

(6) *Substantial Arguments and Type Jumps*

We have already noted the contention that the final justification for the step from C to D is the backing (B). The warrant (W) does justify the inference, but it in turn rests upon, and has no more factual content than, B. If the argument from D to C is a substantial one, then D and B does not entail C. Thus, if we hold that all sound arguments are sound analytic arguments (i.e. arguments in which the premises entail the conclusion), then we have to conclude that there is something wrong with the step from D to C. This is a mistake: there are good and bad analytic arguments and good and bad substantial arguments, but a substantial argument is not a bad argument just because it is not analytic.

The following are examples of the kinds of substantial argument in which Toulmin is interested:

(vi) He is smiling, so he must be happy.

(vii) This will cause a lot of pain and no pleasure, so it must be wrong.

(viii) The sky is clear, so there will be a frost.

In making each of these inferences, it is said, we jump from one logical type to another. To justify an argument from present to future (viii) we produce a warrant: often an empirical law. The law (W) is more than the observation statements (B) on which it is based. First, the step from B to W is often a very difficult one (cf. Kepler's Laws). Secondly, B may justify certain inferences, but, except in very simple cases, until W is formulated we do not know *what* inferences are justified by B. Thirdly, when a law is formulated, we do not jump from knowledge of some to knowledge of all, but, in addition to knowing that, for example, the planets were in such and such positions at such and such times, we do come to know that our observations satisfy certain formulae, and thus we come to know how to predict the positions of the planets. I think that Toulmin would agree with this, for he does say that although a warrant has no more factual content than its backing, it is not a mere repetition of the backing, but rather "a general moral of a practical character" (p. 106).

Although B & D & $\bar{C}$ is not a contradiction, I would suggest that W & D & $\bar{C}$ is. If facts can speak for a warrant, then they can speak against it, and D & $\bar{C}$ is just the sort of fact that does speak against W. Toulmin's point can still be made, but it now has the following more familiar form. Even though W is discredited, B is unscathed. Thus B does not entail W. If we believe that an argument cannot be sound unless it is analytic then we have to conclude that there is something wrong with the step from B to W. We may try to put this right, or we may not. In fact there is nothing to put right: there are good and bad deductive arguments, and good and bad inductive ones, but an inductive argument is not bad just because it is not deductive. Although Toulmin avoids the inductive-deductive dichotomy, it is not clear that this makes much difference to his analysis of arguments, for the advocated investigation of the field-dependent validity of substantial arguments does in the end come down to an examination of the way in which warrants are established in various fields. Nor is it clear that (vi) and (vii) give rise to the same problems as (viii).

(7) *Conclusion*

Because Toulmin repeats himself, he does not cover as much ground as one might expect, and this is unfortunate because his subject is of great interest and his treatment of it original. My main point against him is that he underestimates the importance of formal logic. In some places he takes for granted the ideas of logicians whose work he dismisses as irrelevant, while in others, when dealing with modal qualifiers for example, he makes mistakes that he could have avoided if he had paid more attention to work already done. The kind of analysis of arguments that is suggested in this book is a development in logic rather than a substitute for it.

In confining my remarks to what is said to be crucial in this book, I have omitted a great deal that is interesting and provocative. Indeed, if it is taken, as it is offered, as raising problems rather than solving them (p. 1), then *The Uses of Argument* will be a good starting point for the discussion of a large number of topics.

C. F. PRESLEY.

ETHICS AND THE MORAL LIFE. By Bernard Mayo. London, Macmillan, 1958. viii, 238 p. 34s. 9d. (Australian).

This book is a careful exposition and exploration of the view that moral principles are essentially "universal third-person imperatives" ("let everyone . . . "): imperatives which are, paradoxically, both in the third person and addressed primarily to oneself.

Mr. Mayo is, as I should say, a subjectivist; or, as he would say, a relativist. He prefers to keep the term "subjectivism" for the view that moral judgments express one's likes and dislikes: that "I approve of capital punishment" is like "I like rice pudding". Objectivism, on the other hand, is the view that it is like: "Rice pudding is nutritious". The truth, Mayo tells us, is that it is like the one in some ways and like the other in other ways. Approving is different from liking because it commits one to advancing reasons for one's approval: and the reasons must take the form of principles which are "universalisable in three dimensions". That is to say, they must apply, not just to a particular action, but to all actions of the same class; they must, secondly, apply "not only to me but to you, not only to you but to me, not only to us but to everybody"; and, thirdly, "a moral judgment must be 'universalisable' in the sense that others besides the speaker are assumed to share it". (p. 91). It is this third feature that makes argument possible about morals, as it is not possible about tastes or feelings: when two people argue about whether an action is right, they are asking whether it does or does not follow from a principle which both are assumed to share. To this extent moral utterances resemble statements, since the criteria for empirical assertions are also assumed to be accepted by everyone; but objectivists are at fault, all the same, because they ignore the extent to which moral utterances function as imperatives.

Mayo claims, then, to have settled the question at issue between subjectivists and objectivists, and to have steered cautiously between them. Both, he thinks, are mistaken. But he remains a relativist because, he says, the consensus implied by moral utterances does not actually exist. In disputes about matters of fact, there is always an implied appeal to the evidence of the senses; and we all do accept that, though of course we may differ about what that evidence points to. But in moral disputes it may sometimes happen that each party is implicitly appealing to a principle the other does not accept. In morals, then, there is a dissensus as well as a consensus. This is, Mayo tells us, no accident; he argues, indeed, that morality *implies* a dissensus just as it implies a consensus. To accept a moral rule is not only to commit oneself to a particular kind of action in a particular set of circumstances; it is also to join forces with others who accept the same rule and to "take sides" against anyone who either breaks the rule or challenges it. In making a moral judgment one is, of course, simply applying the criteria one *does* accept; and what is "right" by the standards of one "side" may well be "wrong" by the standards of the other.

It seems clear from this that the reasoned argument that is possible between those who accept the same ultimate moral principles will be very different from the disputes that will occur between those who do not. And this, I should have thought, is what subjectivists mean to assert when they say that moral utterances express emotions or desires: that when we come to consider these ultimate moral principles we find that we can only accept or reject them, much as our palate accepts or rejects rice pudding. And this does seem to be Mayo's position, too. His distinction between what we like and what we approve seems identical with Russell's distinction between personal desires (like a taste for rice pudding) and impersonal desires (like a preference for a world in which there is no capital punishment). The point about impersonal desires, according to Russell, is that we want others to have them too: and it is these desires that moral utterances express. A subjectivism of this type would seem to allow sufficiently for what Mayo says about the "three dimensions of universality". Of course, there is no reason why Mayo should not use the term "relativism" for a position of this kind, and reserve "subjectivism" for a cruder (but surely seldom held?) view which refuses to recognise *any* difference between personal and impersonal desires. But he should not assume that, in conceding the strength of the objectivist case against "subjectivism", he he has said all that he need say about their case against "relativism". It may, indeed, be possible to answer the objectivist arguments; but I doubt if Mayo has done so.

Having put the case for relativism in Part One ("Moral Discourse") Mayo goes on, in Part Two ("The Moral Life") to elaborate the ethics of commitment. Two of the four chapters in this Part are concerned with the "authority" of conscience. In using this term, Mayo points out, we are using a political analogy; and he goes on to discuss the various ways in which both persons and rules may be said to have authority. His conclusion is that: " 'Conscience' is not the name of any faculty or psychical component. Its function is to figure in expressions which refer to the issuing of self-directed commands in accordance with moral rules which we accept as authoritative." (p. 171). Moreover, "what is peculiar about moral authority is not that it is internal but that it is, in an important sense, unchallengeable. All other sources of authority can be challenged . . . One can, by suitable acts of defiance, reject the authority of the State, the Church, the M.C.C. or any other organisation . . . But in practice one usually rejects a certain authority for what one regards as good reasons; and the repudiation takes the form of a preference for some other authority. One may reject the authority of one's schoolmistress who forbids make-up in favour of one's parent who permits it; one may reject the authority of the state which orders training in the art of warfare in favour of one's pacifist convictions . . . Either all our various sources of authority are recognised only provisionally, that is, we regard none of them as beyond challenge; or else there must be some one source of authority which we recognise as unchallengeable, and by reference to which all other authorities can be challenged. This second possibility must be the case if there really are moral principles, as distinct from other kinds of principle. We regard man as a moral agent, only so long as we regard him as having the right to set himself up against all external authorities whatever, and to make up his own mind what he ought to do, irrespective of what he may be commanded to do" (pp. 169-170).

What this amounts to, I think, is that a moral consideration is (by definition) whatever consideration "weighs most" with an individual. There must be some reasons which we regard as final and these must be self-commands. Even if a man feels that he ought not to set up his own conscience against the authority of the Church, the State, or the M.C.C., he is accepting as final the rule that "one ought not to set up one's own conscience, etc." So that there is at least one self-command which he takes to be authoritative, even if it is merely the command to do whatever someone else commands. Whatever considerations an individual accepts as final, these constitute his morality.

In Part Three ("Towards a New Humanism") Mayo suggests that the ethics of rules and principles constitute only a part of the moral life. There is the ethics of "goodness" as well as of "duty": of "being" as well as of "doing". As well as the question "What shall I do?", to which the answer is: "Observe such-and-such a principle", there is the question "What shall I be?", to which the answer is: "Be like so-and-so", where so-and-so is either an actual man (Socrates, St. Francis, Napoleon) or an ideal type (Plato's "just man", "the good communist", "the hundred percent American"). Mayo has two main points here, I think. One is the familiar one that the system of principles one accepts as final constitute "a way of life". The other is that such a way of life is not fully described when it is thought of as a system of principles: it is rather the expression of a set of dispositions, or a "character".

In the book as a whole Mayo is obviously putting forward an ethical position that is now familiar and that has caused (and deserves) a good deal of discussion: discussion to which this book makes a real contribution. One cannot consider this position fully in a review; but Mayo's presentation of it suggests two comments.

(1) It is not clear that the "dissensus" he talks about is bound up with the meaning of "morality". Certainly moral principles presuppose conflict, including conflict within oneself. No doubt to accept a moral rule is to "side against" those who break the rule, including oneself when one breaks it. But to break a rule, as Mayo himself points out, is not to challenge it. Moral principles do presuppose the possibility of their being broken; but not necessarily of their being challenged. It may

well be a fact that any given moral principle is rejected by someone; but it does not follow from the nature of moral principles that this must be the case.

(2) In effect, Mayo advances two different definitions of "moral", which may conflict. In Part One we are told that moral principles are those which are universalisable in the three "dimensions" he specifies; in Part Two that moral principles are those which any given individual takes as final. Now each of these corresponds to an accepted use of the terms "moral" and "morality". We may say of a selfish man, for example, either: "With him selfish considerations override moral ones"; or, "His morality is a purely selfish one". But we cannot conclude, as I think Mayo and others of this school do, that one ought never to accept as final principles that are not universalisable. We may well want to assert this and it may well be true; but we cannot regard it as following from the very meaning of moral terms. To think that we can is to confuse these two uses of "moral". The point is even clearer if it is put in terms of Russell's "personal" and "impersonal" desires. We may, if we like (and with considerable support from usage) define "moral principles" as "the expression of impersonal desires"; but we cannot conclude from this that we ought always to prefer impersonal desires to personal ones.

D. H. MONRO.

AN INTRODUCTION TO THE PHILOSOPHY OF EDUCATION. By D. J. O'Connor. London: Routledge & Kegan Paul, 1957. 148 p. 10s. 6d. (U.K.).

Too many philosophers nowadays treat education and its problems as if they were merely of secondary importance. Academic teachers are, on the whole, prone to be oblivious to the fact that they are also pedagogues. And thus it is only natural that they do not consider the philosophy of education as being worthy of a rigorous inquiry.

Professor O'Connor, though first and foremost a logician, has dealt with the philosophy of education from the point of view of a philosophic analyst. I think he has done future teachers a great service by his brief and lucid definition of the function of philosophic investigation in general. No doubt, to the uninitiated his unromantic, unmetaphysical treatment will come as a shock. However, there is not the slightest evasion of central issues throughout the whole discussion. On the contrary, it is one of the great virtues of O'Connor's book that it challenges established common notions about the aims of education.

What is the goal of all educationalists? How can it be most effectively attained? In order to answer these two vital questions the reader is introduced to a close scrutiny of value judgments, of the meaning of 'theory', of scientific methodology. Some formulations may antagonize even a reader who concurs, on the whole, with O'Connor's general outlook. "We have the same reason for rejecting metaphysics as we have for rejecting witchcraft, astrology or phrenology; it cannot do what it claims to do" (p. 114). And not every votary of logical empiricism will submit to the dogmatic statement that the outcome of philosophizing "has the negative effect of an intellectual antiseptic . . . " (p. 113). On the other hand, one need not be a militant atheist to accept his claim that we have no reason to believe "that there is a connexion of a logical kind between the rules of morality and certain theological statements such that the former are entailed by the latter" (p. 128).

The author regards it as one of the primary ends of education to make man critical. Such questions as vocational training and the awakening of the student's desire for knowledge are adequately appraised. It is also obvious that O'Connor relegates the most essential activities of educational practice to psychology, sociology and the biological sciences. He does not attempt to develop an autonomous educational philosophy. Rather he insists that the true problems of educational theory are universal philosophic problems. For instance, an educator ought to realize that moral judgments are not genuine statements, but mere expressions of approval or disapproval. A philosophy of education has to inquire into the nature of truth, certainly, the concept of mind, the objective criteria for propositions and so forth, no less than any theory of knowledge proper.

As is to be expected, O'Connor deplores the fact that metaphysical doctrines have exerted their dangerous influence not only upon the aims, but also upon the methods, of education. Even those of us who are not avowed followers of fashionable linguistic analysis will agree with the author that a careful study of the relations between the expressive, informative and persuasive uses of language is a necessary prerequisite before we try to find out how value judgments can be justified.

The professional educationalist will, to be sure, vehemently reject O'Connor's lapidary assertion that "the problems of the philosophy of education, if pressed far enough, become the traditional problems of philosophy" (p. 140) ; or that, as stated above, there is only philosophy, but no philosophy of education. I for one do not fully share O'Connor's final conclusion. Yet I have to admit that there is hardly a scintilla of evidence for a refutation of his argument.

All in all, this provocative and often incontrovertibly convincing essay is an incisive piece of work and, though not always cautious, inexorable in its findings. The significance of this book is a fact which this reviewer is only too glad to acknowledge. It is a commonplace that criticism can desiccate. O'Connor's approach brings fresh waters to the roots of educational theory.

WOLFGANG YOURGRAU.

BOOKS RECEIVED

(Mention in this list neither precludes nor guarantees later review.)

Brain, Russell. Science, philosophy and religion; the twelfth Arthur Stanley Eddington Memorial Lecture, 24 February 1959. Cambridge University Press, 1959. iv, 31 p. 4s. 6d. (U.K.) Paper covers.

Briggs, Michael H. Handbook of philosophy. New York, Philosophical Library, 1959. 214 p. $4.75.

Brief definitions of philosophical terms for the beginning student.

Dewey, John. Moral principles in education. New York, Philosophical Library, 1959. xi, 61 p. $2.75.

A reprint.

Gibson, John M. Soldiers of the word; the story of the American Bible Society. New York, Philosophical Library, 1958. 304 p. $3.75.

Gregory, C. C. L. and **Kohsen**, Anita. The O-structure; an introduction to psychophysical cosmology. Church Crookham, Institute for the Study of Mental Images, 1959. 141 p. 21s. (U.K.)

An attempt to construct a world-model which will allow for recent developments in physics, biology, parapsychology, and information theory.

Harris, Errol E. Revelation through reason; reason in the light of science and philosophy. London, Allen and Unwin, 1959. 123 p. 15s. (U.K.)

"Religion and science (are) not really two different departments of human experience, but . . . one and the same thing. Moreover, consideration of the trends observable in modern scientific thought leads to a view of the world which, so it appears to me, necessitates belief not only in God but in such a God as would justify the main doctrine of Christianity: that Jesus is the revelation in human form of the divine." Preface. Originally Terry lectures at Yale.

Halmos, Paul and **Iliffe**, Alan, eds. Readings in general psychology. London, Routledge & Kegan Paul, 1959. ix, 251 p. 25s. (U.K.)

Hawley, Don. The nature of things. New York, Philosophical Library, 1959. 187 p. $3.75.

"A logical destruction of the concept of materialism . . . a bold challenge against . . . spiritual pessimism." Blurb. Argues, for example, that the atom bomb was foretold in the Book of Revelations.

Heisenberg, Werner. Physics and philosophy; the revolution in modern science. London, Allen and Unwin, 1959. 176 p. 15s. (U.K.)

Discusses the philosophical implications of quantum theory.

Heywood, Jean S. Children in care; the development of the service for the deprived child. London, Routledge & Kegan Paul, 1959. x, 229 p. 25s. (U.K.)

HEYWOOD, Rosalind. The sixth sense; an inquiry into extra-sensory perception. London, Chatto & Windus, 1959. 224 p. 21s. (U.K.)

Describes and discusses the principal experimental evidence for ESP.

HIRST, R. J. The problems of perception. London, George Allen and Unwin Ltd., 1959. x, 330 p. 30s. (U.K.)

Examines critically various theories of perception and offers an explanatory hypothesis, described by the author as "a version of monism which may be classed as a Double Aspect Theory . . . an Identity Hypothesis".

HOOK, Sidney, ed. Psychoanalysis, scientific method and philosophy; a symposium. New York University Press, 1959. xiii, 370 p. $5.

The proceedings of the second annual New York University Institute of Philosophy, 1958, attended by philosophers, psychologists, psychoanalysts and sociologists. The main discussion is about the scientific status of psychoanalysis, with papers by Nagel, Hartmann and Kubie, and comments by Arlow, Hook, Pap, Hospers and others. There is also a psychoanalytic "explanation" of philosophy, by Laserowitz, with an angry retort by Donald Williams, and a rather more effective one by Flew; and papers on the contribution of psychoanalysis to the social sciences, by Kardiner, van den Haag, and Inkeles.

HULL, L. W. H. History and philosophy of science; an introduction. London, Longmans, Green, 1959. xi, 340 p. 43s. 6d. (U.K.)

Mainly history; but "the theme is the relation of science to thought rather than action" (Preface). Hence there is some discussion of Plato, Aristotle, Locke, Berkeley and Hume.

KRIKORIAN, Yervant H. and EDEL, Abraham. Contemporary philosophic problems; selected readings. New York, Macmillan Co., 1959. xi, 712 p. $7.

There are five sections, on "Meaning", "Knowledge", "Being", "Value", and "Vision and Action". A mixed lot of authors, including (e.g.) Gandhi, Heidegger, Spengler and Niebuhr as well as Peirce, Broad, Ayer, Russell, Bosanquet and Ryle.

LAMONT, Corliss. The illusion of immortality. 3rd ed. New York, Philosophical Library, 1959. xv, 303 p. $3.95.

First published 1934, but now revised and rewritten. Presents the case against continued existence after death.

LAZOWICK, Frank E. The science of philosophy. New York, Philosophical Library, 1959. ix, 379 p. $6.

"The seven Dimensions of dynamic Process and of Being characterize all objects", viz., Faith, Wisdom, Justice, Might, Freedom, Love and Beauty.

MARX, Karl. A world without Jews; edited, with an introduction, by Dagobert D. Runes. New York, Philosophical Library, 1959. xii, 51 p. $2.75.

Claims to be "the first unexpurgated English language publication" of two short papers, "The Jewish Question" and "The Capacity of Today's Jews and Christians to Become Free", published by Marx in 1843 in reply to Dr. Bruno Bauer.

MAYS, W. The philosophy of Whitehead (Muirhead Library of Philosophy.) London, Allen & Unwin, 1959. 259 p. 25s. (U.K.)

A commentary on, and re-interpretation of, *Process and Reality*. Argues that "the key to the understanding of Whitehead is to be found in his earlier philosophy of science".

MILES, T. R. Religion and the scientific outlook. London, Allen and Unwin, 1959. 224 p. 21s. (U.K.)

Argues that any conflict between science and religion can be avoided by regarding religious language as "the language of parable", which makes no factual assertions about the nature of the universe. "To accept the theistic parable is to commit ourselves to a particular way of life."

MORTIMER, Ernest. Blaise Pascal; the life and work of a realist. London, Methuen, 1959. 240 p. 21s. (U.K.)

ORTEGA Y GASSET, José. Man and crisis; translated by Mildred Adams. London, Allen and Unwin, 1959. 217 p. 16s. (U.K.)

A study of ages of transition, such as the Renaissance, or our own time.

PEIRCE, C. S. Collected papers of Charles Sanders Peirce, ed. by Arthur W. Burks: volume vii, Science and philosophy; volume viii, Reviews, correspondence and bibliography. Cambridge, Mass., Harvard University Press; Oxford University Press, 1958. xiv, 415 p. + xii, 352 p. £4 4s. ea. (Australian).

"Many of the manuscripts have never been published before, and the previously published material . . . is widely scattered in a number of journals". Blurb.

SCHWEITZER, Albert. The light within us. New York, Philosophical Library, 1959. 58 p. $2.75.

Brief aphorisms selected from Schweitzer's writings.

STRAWSON, P. F. Individuals; an essay in descriptive metaphysics. London, Methuen, 1959. 255 p. 25s. (U.K.)

"The first part aims at establishing the central position which material bodies and persons occupy among particulars in general . . . In the second part of the book the aim is to establish and explain the connexion between the idea of a particular in general and that of an object of reference or logical subject." Introduction.

TOMKINS, Silvan S. and **MINER**, John B. PAT interpretation: scope and technique. New York, Springer, 1959. 184 p. $5.50.

A "picture arrangement test" for assessing personality.

WEITZ, Morris, ed. Problems in æsthetics; an introductory book of readings. New York, Macmillan Co., 1959. ix, 697 p. $6.90.

"I have tried to present the best discussions of the main problems of æsthetics regardless of their dates of composition . . . I have organized the readings around certain sets of basic problems." Preface.

ZUURDEEG, Willem F. An analytical philosophy of religion. London, Allen and Unwin, 1959. 320 p. 30s. (U.K.)

Claims to be an analysis of religious language which attempts to be "as unbiased as humanly possible", and hence not, like other recent works, "a defence (or attack) of the Christian claim in terms of the new philosophy".

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